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PHYSIOLOGICAL RESEARCHES.

LONDON:
SPOTTISWOODES and SHAW,
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PHYSIOLOGICAL RESEARCHES.

BY

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COLLECTED AND REPUBLISHED FROM THE
"PHILOSOPHICAL TRANSACTIONS."

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1851.

ADVERTISEMENT.

THE following papers, which were published many years ago in the Transactions of the Royal Society, are now for the first time collected, and republished in a separate volume.

I have availed myself of the opportunity thus afforded to offer, in the form of notes, some additional observations, which appear in some instances to confirm, while in others they have led me to modify, the conclusions at which I had arrived formerly.

Having been long engaged in the duties of an arduous profession, I have not had it in my power to continue the pursuit of those physiological inquiries to which I was able to devote a considerable portion of my time during the early part of my professional life. However,

whatever loss this may have occasioned to myself, it has caused no loss to the public. A part of these inquiries at least, has been most successfully pursued by others, and especially the labours of Orfila, Christison, and Taylor have extended our knowledge of the destructive agency of poisons far beyond the limits of my original investigations.

October 23. 1851.

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PHYSIOLOGICAL RESEARCHES.

I.

The Croonian Lecture, on some Physiological Researches, respecting the Influence of the Brain on the Action of the Heart, and on the Generation of Animal Heat.

[From the Philosophical Transactions for 1811.]

HAVING had the honour of being appointed by the President of the Royal Society to give the Croonian Lecture, I trust that the following facts and observations will be considered as tending sufficiently to promote the objects for which that Lecture was instituted. They appear to be of some interest as throwing light on the mode in which the influence of the brain is necessary to the continuance of the action of the heart; and also as shewing that the integrity of the nervous system is in some way necessary to the production of animal heat.

In making experiments on animals to ascer-

tain how far the influence of the brain is necessary to the action of the heart, I found that in an animal in which the spinal chord had been divided in the upper part of the neck, respiration was immediately suspended, while the heart still continued to contract circulating dark-coloured blood, and that, in some instances, from ten to fifteen minutes elapsed before the heart's action had entirely ceased. I further found that when the head was removed, the divided blood vessels having been secured by a ligature, the circulation still continued, apparently unaffected by the entire separation of the brain. These experiments confirmed the observations of Mr. Cruikshank* and M. Bichat†, that the brain is not directly necessary to the action of the heart, and that when the functions of the brain are destroyed, the circulation ceases only in consequence of the suspension of respiration. This led me to conclude, that, if respiration were produced artificially, the heart would continue to contract for a still longer period of time after the removal of the brain. The truth of this conclusion was ascertained by the following experiment.

EXPERIMENT I.

I divided the spinal chord of a rabbit in the space between the occiput and atlas, and having made an opening into the trachea, fitted into it

* Philosophical Transactions, 1795.

† Récherches Physiologiques sur la Vie et la Mort.

a tube of elastic gum, to which was connected a small pair of bellows, so constructed that the lungs might be inflated, and then allowed to empty themselves. By repeating this process once in five seconds, the lungs being each time fully inflated with fresh atmospheric air, an artificial respiration was maintained. I then secured the blood-vessels in the neck, and removed the head, by cutting through the soft parts above the ligature, and separating the occiput from the atlas. The heart continued to contract apparently with as much strength and frequency as in a living animal. I examined the blood in the different sets of vessels, and found it dark-coloured in the venæ cavæ and pulmonary artery, and of the usual florid red colour in the pulmonary veins and aorta. At the end of 25 minutes from the time of the spinal chord being divided, the action of the heart became fainter, and the experimenws put an end to.

With a view to promote the enquiry instituted by the Society for promoting the Knowledge of Animal Chemistry respecting the influence of the nerves on the secretions*, I endeavoured to ascertain whether they continued after the influence of the brain was removed. In the commencement of the experiment I emptied the bladder of its contents by pressure. At the end of the experiment the bladder continued empty.

* Philosophical Transactions for 1809.

This experiment confirmed the expectation that the action of the heart might be made to continue after the brain was removed, but at the same time tended to show that, under these circumstances, there was a suspension of the secretion of urine. It appeared, however, desirable that the experiment should be repeated, and it occurred to me that in repeating it, it would be well to ascertain whether, under the same circumstances, the animal heat was maintained at the natural standard.

EXPERIMENT II.

The subject of this experiment was a middle-sized dog. The temperature of the room was 63° of Fahrenheit's thermometer. By having previously secured the carotid and vertebral arteries, I was enabled to remove the head with little or no hæmorrhage. The artificial respirations were made about 24 times in a minute. The heart acted with regularity and strength.

At the end of 30 minutes from the time of the spinal chord being divided, the heart was felt through the ribs contracting 76 times in a minute.

At 35 minutes the pulse had risen to 84 in a minute.

At one hour and 30 minutes the pulse had risen to 88 in a minute.

At the end of two hours it had fallen to 70, and at the end of two hours and a half to 35 in a minute, and the artificial respiration was no longer continued.

By means of a small thermometer with an exposed bulb, I measured the animal heat at different periods.

At the end of an hour the thermometer in the rectum had fallen from 100° to 94°.

At the end of two hours a small opening having been made in the parietes of the thorax, and the bulb of the thermometer placed in contact with the heart, the mercury fell to 86°, and half an hour afterwards in the same situation it fell to 78°.

In the beginning of the experiment I made an opening into the abdomen, and having passed a ligature round each ureter about two inches below the kidney, brought the edges of the wound in the abdomen together by means of sutures. At the end of the experiment no urine was collected in the ureters above the ligatures.

On examining the blood in the different vessels, it was found to be of a florid red colour in the arteries, and of a dark colour in the veins, as under ordinary circumstances.

During the first hour and a half of the experiment there were constant and powerful contractions of the muscles of the trunk and extremities, so that the body of the animal was moved in a very remarkable manner on the table on which it lay, and twice there was a copious evacuation of fæces.

EXPERIMENT III.

The experiment was repeated on a rabbit. The temperature of the room was 60°. The re-

spirations were made from 30 to 35 times in a minute. The action of the heart at first was strong and frequent: but at the end of one hour and 40 minutes the pulse had fallen to 24 in a minute.

The blood in the arteries was seen of a florid red, and that in the veins of a dark, colour.

A small opening was made in the abdominal muscles, through which the bulb of a thermometer was introduced into the abdomen, and allowed to remain among the viscera.

At the end of an hour the heat in the abdomen had fallen from 100° to 89°. At the end of an hour and forty minutes in the same situation the heat had fallen to 85°, and when the bulb of the thermometer was placed in the thorax in contact with the lungs the mercury fell to 82°.

It has been the generally received opinion that the heat of warm-blooded animals is dependent on the chemical changes produced on the blood by the air in respiration. In the two last experiments the animals cooled very rapidly, notwithstanding that the blood appeared to undergo the usual changes in the lungs, and I was therefore induced to doubt whether the above mentioned opinion respecting the source of animal heat is altogether correct. No positive conclusions, however, could be deduced from these experiments. If animal heat depends on the changes produced on the blood by the air in respiration, its being kept up to the natural standard must depend on there being a certain quantity of air inspired, and a certain quantity of

blood propelled through the lungs in a given space of time: in other words, it must bear a relation to the fulness and frequency of the pulse, and the completeness of the respiration. It therefore became necessary to pay particular attention to these circumstances.

EXPERIMENT IV.

The experiment was repeated on a dog of a small size, whose pulse was from 130 to 140 in a minute, and whose respirations, as far as I could judge, were performed from 30 to 35 times in a minute.

The temperature of the room was 63°. The heat in the rectum of the animal at the commencement of the experiment was 99°. The artificial inspirations were made to correspond as nearly as possible to the natural inspirations, both in fulness and frequency.

At 20 minutes from the time of the division of the spinal chord, the heart acted 140 times in a minute with as much strength and regularity as before: the heat in the rectum had fallen to 96½°.

At 40 minutes the pulse was still 140 in a minute: the heat in the rectum 92½°.

At 55 minutes the pulse was 112, and the heat in the rectum 90°.

At one hour and 10 minutes the pulse beat 90 in a minute, and the heat in the rectum was 88°.

At one hour and 25 minutes the pulse had fallen to 30, and the heat in the rectum to 85°.

The bulb of the thermometer having been placed within the pericardium, the mercury stood at 85°, but when among the viscera of the abdomen it rose to 87½°.

During the experiment there were frequent and violent contractions of the voluntary muscles, and an hour after the experiment was begun, there was an evacuation of fæces.

EXPERIMENT V.

The experiment was repeated on a rabbit, whose respirations, as far as I could judge, were from 30 to 40 in a minute, and whose pulse varied from 130 to 140 in a minute. The temperature of the room was 57°. The heat in the rectum, at the commencement of the experiment, was 101½°. The artificial respirations were made to resemble the natural respirations as much as possible, both in fulness and frequency.

At 15 minutes from the time of the spinal chord being divided, the heat in the rectum had fallen to 98½°.

At the end of half an hour the heart was felt through the ribs, acting strongly 140 times in a minute.

At 45 minutes the pulse was still 140; the heat in the rectum was 94°.

At the end of an hour the pulse continued 140 in a minute; the heat in the rectum was 92°; among the viscera of the abdomen 94°; in the thorax, between the lungs and pericardium, 92°.

During the experiment the blood in the femoral artery was seen to be of a bright florid colour, and that in the femoral vein of a dark colour, as usual.

The rabbit voided urine at the commencement of the experiment; at the end of the experiment no urine was found in the bladder.

EXPERIMENT VI.

I procured two rabbits of the same colour, but one of them was about one-fifth smaller than the other. I divided the spinal chord of the larger rabbit between the occiput and atlas. Having secured the vessels in the neck, and removed the head, I kept up the circulation by means of artificial respiration as in the former experiments. The respirations were made as nearly as possible similar to natural respirations.

In 23 minutes after the spinal chord had been divided, the pulse was strong, and 130 in a minute: the bulb of the thermometer being placed among the viscera of the abdomen, the mercury stood at 96°.

At 34 minutes the pulse was 120 in a minute; the heat in the abdomen was 95°.

At the end of an hour the pulse could not be felt, but, on opening the thorax, the heart was found acting, slowly and feebly. The heat in the abdomen was 91°; and between the lobes of the right lung 88°.

During the experiment, the blood in the ar-

teries and veins was seen to have its usual colour.

In this therefore, as in the preceding experiments, the heat of the animal fell rapidly, notwithstanding the continuance of the respiration. In order to ascertain whether any heat at all was generated by this process, I made the following comparative experiment. The temperature of the room being the same, I killed the smaller rabbit by dividing the spinal chord between the occiput and atlas. In consequence of the difference of size, *cæteris paribus*, the heat in this rabbit ought to diminish more rapidly than in the other; and I therefore examined its temperature at the end of 52 minutes, considering that this would be at least equivalent to examining that of the larger rabbit at the end of an hour. At 52 minutes from the time of the smaller rabbit being killed, the temperature among the viscera of the abdomen was 92°, and between the lobes of the right lung it was 91°. From this experiment, therefore, it appeared not only that no heat was generated in the rabbit, in which the circulation was maintained by artificial respiration, but that it even cooled more rapidly than the dead animal.

At the suggestion of Professor Davy, who took much interest in the enquiry, I repeated the foregoing experiment on two animals, taking pains to procure them more nearly of the same size and colour.

EXPERIMENT VII.

I procured two large full grown rabbits, of the same colour, and so nearly equal in size, that no difference could be detected by the eye.

The temperature of the room was 57°, and the heat in the rectum of each rabbit previously to the experiment was 100½°.

I divided the spinal chord in one of them, produced artificial respiration, and removed the head, after having secured the vessels in the neck. The artificial respirations were made about 35 times in a minute.

During the first hour, the heart contracted 144 times in a minute.

At the end of an hour and a quarter the pulse had fallen to 136 in a minute, and it continued the same at the end of an hour and a half. At the end of an hour and 40 minutes the pulse had fallen to 90 in a minute, and the artificial respiration was discontinued.

Half an hour after the spinal chord had been divided, the heat in the rectum had fallen to 97°.

At 45 minutes it was 95½°.

At the end of an hour the heat in the rectum was 94°.

At an hour and a quarter it was 92°.

At an hour and a half it was 91°.

At an hour and 40 minutes, the heat in the rectum was 90½°, and in the thorax, within the bag of the pericardium, it was 87½°.

The temperature of the room being the same,

the second rabbit was killed by dividing the spinal chord, and the temperature was examined at corresponding periods.

Half an hour after the rabbit was killed, the heat in the rectum was 99°.

At 45 minutes it had fallen to 98°.

At the end of an hour the heat in the rectum was $96\frac{1}{2}$°.

At an hour and a quarter it was 95°.

At an hour and a half it was 94°.

At an hour and 40 minutes the heat in the rectum was 93°, and within the pericardium it was $90\frac{1}{2}$°.

The following table will shew the comparative temperature of the two animals at corresponding periods.

Time.	Rabbit with artificial Respiration.		Dead Rabbit.	
	Thermometer in the Rectum.	Thermometer in the Pericardium.	Thermometer in the Rectum.	Thermometer in the Pericardium.
Before the experiment	$100\frac{1}{2}$	- -	$100\frac{1}{2}$	
30 min.	97	- -	99	
45 „	$95\frac{1}{2}$	- -	98	
60 „	94	- -	$96\frac{1}{2}$	
75 „	92	- -	95	
90 „	91	- -	94	
100 „	$90\frac{1}{2}$	$87\frac{1}{2}$	93	$90\frac{1}{2}$

In this experiment, the thorax, even in the dead animal, cooled more rapidly than the abdomen. This is to be explained by the difference in the bulk of these two parts. The rabbit in

which the circulation was maintained by artificial respiration, cooled more rapidly than the dead rabbit, but the difference was more perceptible in the thorax than in the rectum. This is what might have been expected, if the production of animal heat be not immediately dependent on respiration, since the cold air by which the lungs were inflated, must necessarily have abstracted a certain quantity of heat from the whole body, and a still larger quantity from the parts with which it came into actual contact.

Still, it was plain that some animal heat might have been generated, though so small in quantity as not to counterbalance the cooling powers of the air thrown into the lungs. It is difficult or impossible, to ascertain with perfect accuracy, what effect cold air thrown into the lungs would have on the temperature of an animal under the circumstances of the last experiment, independently of any chemical action on the blood: since, if no chemical changes were produced, the circulation could not be maintained, and if the circulation ceased, the cooling properties of the air must be more confined to the thorax, and not communicated in an equal degree to the more distant parts. The following experiment, however, was instituted, as likely to afford a nearer approximation to the truth, than any other that could be devised.

EXPERIMENT VIII.

I procured two rabbits of the same size and colour. The temperature of the room was 64°.

I killed one of them by dividing the spinal chord, and immediately, having made an opening into the left side of the thorax, I tied a ligature round the base of the heart, so as to stop the circulation. The external wound was closed by a suture. An opening was then made into the trachea, and the apparatus for artificial respiration having been fitted into it, the lungs were inflated, and then allowed to collapse as in the former experiment, about 36 times in a minute. This was continued for an hour and a half, and the temperature was examined at different periods. The temperature of the room being the same, I killed the second rabbit in the same manner, and measured its temperature at corresponding periods. The comparative temperature of the two dead animals, under these circumstances, will be seen in the following table.

Time.	Dead Rabbit whose Lungs were Inflated.		Dead Rabbit whose Lungs were not Inflated.	
	Thermometer in the Rectum.	Thermometer in the Thorax.	Thermometer in the Rectum.	Thermometer in the Thorax.
Before the experiment	100	- -	100	
30 min.	97	- -	98	
45 „	95½	- -	96	
60 „	94	- -	94½	
75 „	92½	- -	93	
90 „	91	86	91½	88½

In this last experiment, as may be seen from the above table, the difference in the temperature of the two rabbits, at the end of an hour and a

half, in the rectum, was half a degree, and in the thorax two degrees and a half; whereas, in the preceding experiment, at the end of an hour and 40 minutes, the difference in the rectum was $2\frac{1}{2}$ degrees, and in the thorax 3 degrees. It appears, therefore, that the rabbit in which the circulation was maintained by artificial respiration, cooled more rapidly, on the whole, than the rabbit whose lungs were inflated after the circulation had ceased. The explanation is sufficiently obvious. In the last case the cold air was always applied to the same surface, but in the former it was also applied to fresh portions of blood, by the circulation of which its cooling powers were communicated to the more distant parts of the body.

I have selected the above from a great number of similar experiments, which it would be needless to detail. It is sufficient to state, that the general results were always the same; and that whether the pulse was frequent or slow, full or small, or whether the respirations were frequent or otherwise, there was no very perceptible difference in the cooling of the animal.

From the whole it appears to me that we may deduce the following conclusions:

1. The influence of the brain is not directly necessary to the action of the heart.

2. When the brain is injured or removed, the

action of the heart ceases, only because respiration is under its influence, and if under these circumstances respiration be artificially produced, the circulation will still continue.

3. When the influence of the brain is removed the secretion of urine is suspended, and no heat is generated; notwithstanding that the functions of respiration, and the circulation of the blood, continue to be performed, and the usual changes in the appearance of the blood are produced in the lungs.

4. When the air respired is colder than the natural temperature of the animal, the immediate effect of respiration may be not to generate, but even to diminish animal heat.*

* In the original publication a postscript was added to the Croonian Lecture, detailing an experiment which shewed that carbonic acid was generated in considerable quantity during the respiration of a decapitated animal. This is omitted on the present occasion, the necessity for introducing it being superseded by the more exact experiments related in the following paper.

II.

Further Experiments and Observations on the Influence of the Brain on the Generation of Animal Heat. Communicated to the Society for promoting the Knowledge of Animal Chemistry, and by them to the Royal Society.

[From the Philosophical Transactions for 1812.]

In the Croonian Lecture for the year 1810, I gave an account of some experiments, which led me to conclude that the production of animal heat is very much under the influence of the nervous system. Some facts which have since fallen under my observation illustrate this subject, and seem to confirm the truth of my former conclusions.

In an animal which is under the influence of a poison that operates by disturbing the functions of the brain, in proportion as the sensibility becomes impaired, so is the power of generating heat impaired also.

If an animal be apparently dead from a poison of this description, and the circulation of the blood be afterwards maintained by means of artificial respiration, the generation of heat is

found to be as completely destroyed, as if the head had been actually removed.

Under these circumstances, if the artificial respiration be kept up until the effects of the poison cease, as the animal recovers his sensibility, so does he also recover the power of generating heat; but it is not till the nervous energy is completely restored, that heat is produced in sufficient quantity to counteract the cold of the surrounding atmosphere.*

In the experiments formerly detailed, as well as in those just mentioned, I observed that the blood underwent the usual alteration of colour in the two systems of capillary vessels, while carbonic acid was evolved from the lungs at each expiration; and hence I was led to believe, that the respiratory function was performed nearly as under ordinary circumstances, and that the usual chemical changes were produced on the blood. It appeared, however, desirable to obtain some more accurate knowledge on these points, and I have therefore instituted a series of experiments, for the purpose of ascertaining the relative quantities of air consumed in breathing, by animals in a natural state, and by animals in which the

* The poison employed in this experiment, should be the essential oil of almonds, or some other, the effects of which speedily subside. If the woorara be employed, so long a time elapses before the poison ceases to exert its influence, that it becomes necessary that the experiment should be made in a high temperature, otherwise the great loss of heat which takes place, is sufficient to prevent recovery.

brain has ceased to perform its office; and I now have the honour of communicating an account of these experiments to the Society.

It has been shown, by Messrs. Allen and Pepys*, first, that every cubic inch of carbonic acid requires exactly a cubic inch of oxygen gas for its formation; secondly, that when respiration is performed by a warm-blooded animal in atmospheric air, the nitrogen remains unaltered, and the carbonic acid exactly equals, volume for volume, the oxygen gas which disappears.

There is therefore reason to believe, that the watery vapour, which escapes with the air in expiration, is not formed from the union of hydrogen with oxygen in the lungs, but that it is exhaled from the mucous membrane of the mouth and pharynx, resembling the watery exhalation which takes place from the peritonæum, and other membranous surfaces, when exposed; and this conclusion appears to be confirmed by the experiments of M. Magendie, lately communicated to the National Institute of Paris.

These circumstances are of importance in the present communication, which they render more simple, as they show, that in order to ascertain the changes produced on the air in respiration, it is only necessary to determine the quantity of carbonic acid given out from the lungs. This becomes an exact measure of the oxygen consumed; and the nitrogen of the air and the watery

* Phil. Trans. 1807, 1808, 1809.

vapour expired need not be taken into the account.

For the purpose of examining the changes produced on the air, by animals breathing under the different circumstances above mentioned, I contrived an apparatus, which, although simple in its structure, thas been found very well to answer the intended purpose. It is represented in the accompanying plate, by referring to which the construction and application of it will be easily understood.

Description of the Apparatus.

A is a wooden stand, in which is a circular groove, three-fourths of an inch in depth, and the same in width.

B is a bell-glass, the rim of which is received in the circular groove of the wooden stand. In the upper part of the bell-glass is an opening, admitting a tube connected with the bladder C.

D is a bottle of elastic gum, having a brass stop-cock, E, connected with it.

F is a silver tube, of which one end is adapted to receive the tube of the stop-cock, E, while the other extremity, making a right angle with the rest of the tube, passes through a hole in the wooden stand, and projects into the cavity of the bell-glass, where it makes a second turn, also at a right angle, and becomes of a smaller diameter. In the upright part of the tube is an opening G.

The tubes are made perfectly air-tight, where

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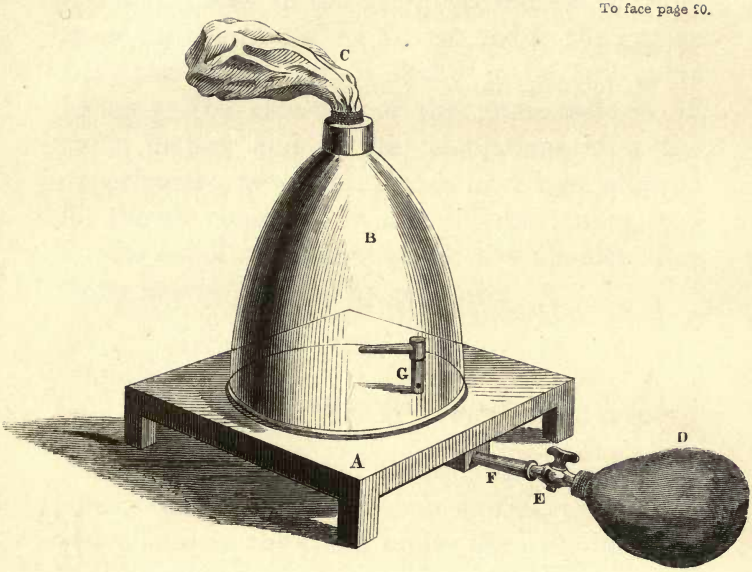

connected with each other and with the rest of the apparatus, and the circular groove is filled with quicksilver.

The capacity of the bell-glass, allowance being made for the rim, which is received in the groove with the quicksilver, is found to be 502 cubic inches. The capacity of the gum-bottle is 52 cubic inches, and in the calculations after the experiments, two cubic inches have been allowed for the air contained in the different tubes, and for the small remains of air in the bladder after being nearly emptied by pressure.

Mode of using the Apparatus.

In order to ascertain the quantity of air consumed under ordinary circumstances, the animal was placed on the stand under the bell-glass, the bladder having been emptied by pressure, and the gum-bottle distended with atmospheric air. During the experiment, by pressing occasionally on the gum-bottle, the air was forced from it into the bell-glass. On removing the pressure, the gum-bottle became filled by its own elasticity with air from the bell-glass. Thus the air was kept in a state of agitation, while the dilatation of the bladder prevented it from being forced through the quicksilver under the edge of the bell-glass. At the end of the experiment, the gum-bottle was completely emptied by pressure, and allowed to be again filled with air from the

bell-glass. This was repeated two or three times, and the air in the bottle was then preserved for examination. The proportion of carbonic acid having been ascertained, and the capacities of the different parts of the apparatus, and the space occupied by the animal being known, the total quantity of carbonic acid formed, and consequently of oxygen consumed, was easily estimated.

When the experiment was made on an animal in whom the functions of the brain were destroyed, and in whom therefore voluntary respiration had ceased, the narrow extremity of the tube was inserted into an artificial opening in the trachea, and the animal being placed under the bell-glass, the lungs were inflated at regular intervals, by means of pressure made on the gum-bottle. The tube being smaller than the trachea, the greater portion of the air in expiration escaped by the side of the tube into the general cavity of the bell-glass, while the gum-bottle filled itself by its own elasticity with air through the opening G. At the end of the experiment, a portion of air was preserved for examination, and the quantity of carbonic acid was estimated in the way already described.

The animals employed in these experiments were of the same species, and nearly of the same size. Attention to these circumstances was judged necessary, that the results might be as little liable to error as possible. The chemical examination of the air, was made by agitating it

in a graduated measure over quicksilver, with a watery solution of potash. My friend Professor Brande gave me his assistance in this part of the present investigation, as he had done in many of my former experiments. It will be observed, that in estimating the proportion of carbonic acid, no allowance has been made for that contained in the atmospheric air; first, because the quantity is so small that the omission can occasion no material error; and, secondly, because the object was to ascertain, not so much the absolute, as the relative quantities, of carbonic acid evolved by animals breathing under different circumstances.

The experiments which I shall first notice, were made on the respiration of animals in a natural state.

EXPERIMENT I.

Thermometer 65°, barometer not noted.

A young rabbit was allowed to remain under the bell-glass during 30 minutes. The respired air at the end of this time was found to contain $\frac{1}{20}$ of carbonic acid.

It was ascertained that the rabbit occupied the space of 50 cubic inches.

The capacity of the bell-glass=502 cubic inches.

That of the gum-bottle=52 cubic inches.

The air in the tubes and bladder=2 cubic inches.

Then $\frac{502+52+2-50}{20}=\frac{506}{20}=25{\cdot}3$. The

rabbit, therefore, in 30 minutes, gave out 25·3 cubic inches of carbonic acid, and consumed the same quantity of oxygen gas, which is at the rate of 50·6 cubic inches in an hour.

EXPERIMENT II.

Thermometer 65°, barometer 30·1 inch.

A somewhat smaller rabbit was allowed to remain under the bell-glass during 30 minutes. The respired air contained $\frac{1}{18}$ of carbonic acid. The animal occupied the space of 48 cubic inches.

$$\frac{502+52+2-48}{18}=\frac{508}{18}=28\cdot22.$$

The carbonic acid evolved, was therefore equal to 28·22 cubic inches in half an hour, which is at the rate of 56·44 cubic inches in an hour.

EXPERIMENT III.

Thermometer 64°, barometer 30·2 inch.

A young rabbit, occupying the space of 48 cubic inches, was allowed to remain under the bell-glass during the same period as in the two former instances. The respired air contained $\frac{1}{18}$ of carbonic acid.

$$\frac{502+52+2-48}{18}=\frac{508}{18}=28\cdot22.$$

The results of this were therefore precisely the same as those of the last experiment.

These experiments were made with great care. The animals did not appear to suffer any inconvenience from their confinement, and their temperature was unaltered.

The next order of experiments was instituted for the purpose of ascertaining the quantity of air consumed by animals in which the circulation of the blood was kept up by means of artificial respiration, after the brain had ceased to perform its functions.

EXPERIMENT IV.

Thermometer 65°, barometer not noted.

Having procured two rabbits of the same size and colour, I divided the spinal chord in the upper part of the neck of one of them. An opening was made in the trachea, and the lungs were inflated at first by means of a small pair of bellows. Two ligatures were passed round the neck, one in the upper, and the other in the lower part, behind the trachea. The ligatures were drawn tight, including every thing but the trachea; and the nerves, vessels, and other soft parts between them were divided with a bistoury. Eight minutes after the division of the spinal chord, the thermometer in the rectum had sunk to 97°. The animal was placed under a bell-glass, and the lungs were inflated by pressing on the gum-bottle about 50 times in a minute. When this process had been continued for 30 minutes, a portion of air was abstracted from the bell-glass, and preserved for examination. The heart was found acting regularly, but slowly, the thermometer in the rectum had fallen to 90°.

The second rabbit was killed by dividing the spinal chord about the same time when the

experiment was begun on the first rabbit. Being in the same temperature, the time was noted when the thermometer in the rectum had fallen to 97°, and it was placed under another bell-glass, that it might be as nearly as possible under the same circumstances as the first rabbit. At the end of 30 minues, the thermometer in the rectum had fallen from 97° to 91°.*

The air respired by the first rabbit contained $\frac{1}{25}$ of carbonic acid. The bulk of the rabbit was found=50 cubic inches.

$$\frac{502+52+2-50}{25}=\frac{506}{25}=20{\cdot}24.$$

20·24 cubic inches of carbonic acid were therefore extricated in 30 minutes, which is at the rate of 40·48 cubic inches in an hour.

The carbonic acid given out in the same space of time, was less than in the former experiments; but it is to be observed, first, that in consequence of the ligatures, the extent of the circulation was diminished; secondly, that in this instance one of the ligatures accidentally slipped, and an ounce of blood was lost in the beginning of the experiment.

As it was desirable to avoid any circumstances which might occasion a difference in the results,

* In measuring the heat of the rectum in these experiments, care is necessary that the thermometer should always be introduced to exactly the same distance from the external parts, otherwise no positive conclusion can be drawn relative to the loss of heat, as the more internal parts retain their heat longer than the superficial.

in the subsequent experiments I employed animals which had been inoculated with the poison of woorara, or the essential oil of almonds; by which means, while the functions of the brain were completely destroyed, the extent of the circulation was undiminished, and the chance of accidental hæmorrhage was avoided.

EXPERIMENT V.

Thermometer 65°, barometer 29·8 inch.

Two rabbits were procured, each occupying the space of 45 cubic inches. They were both inoculated with the woorara poison.

The first rabbit was apparently dead in nine minutes after the application of the poison; but the heart continued to act. The lungs were inflated for about two minutes, by means of a pair of bellows, when the thermometer in the rectum was observed to stand at 98°. The animal was then placed under the bell-glass, and artificial respiration was produced by means of pressure on the gum-bottle, as in the last experiment. At the end of 30 minutes, a portion of air was preserved for examination. The thermometer in the rectum had fallen to 91°. The heart still acted with regularity and strength.

The second rabbit died in a few minutes after the inoculation. The time was noted when the thermometer in the rectum had fallen to 98°, and he was placed under another bell-glass. At the end of 30 minutes, the thermometer in the rectum had fallen to 92°.

The air respired by the first rabbit contained $\frac{1}{20}$ of carbonic acid.

$\frac{502+52+2-45}{20}=\frac{511}{20}=25{\cdot}55$ cubic inches of carbonic acid evolved in 30 minutes, which is at the rate of 51·1 cubic inches in an hour.

EXPERIMENT VI.

Thermometer 66°, barometer 30·1 inch.

Two rabbits, each occupying the space of 48 cubic inches, were inoculated with woorara.

In one of them, when apparently dead, the circulation was kept up by means of artificial respiration. He was placed in the apparatus under the glass-bell, and the lungs were inflated from 50 to 60 times in a minute. At this time the thermometer in the rectum stood at 97°. At the end of 35 minutes, a portion of air was preserved for examination. The thermometer had now fallen to 90°. The heart was still acting regularly.

The second rabbit was allowed to lie dead. When the thermometer in the rectum had fallen to 97°, he was placed under another bell-glass. At the end of 35 minutes, the thermometer had fallen to 90·5°.

The air respired by the first rabbit, contained $\frac{1}{16}$ of carbonic acid.

$\frac{502+2+52-48}{16}=\frac{508}{16}=31{\cdot}75$ cubic inches of

carbonic acid evolved in 35 minutes, which is at the rate of 54·43 cubic inches in an hour.

EXPERIMENT VII.

Thermometer 60°, barometer 30·2 inch.

The experiment was repeated on a rabbit which had been inoculated with the essential oil of bitter almonds. When he was placed under the bell-glass, the thermometer in the rectum stood at 96°. In a few minutes he gave signs of sensibility, and made efforts to breathe; but as these were at long intervals, and not sufficient to maintain life, the artificial respiration was continued. In half an hour he breathed spontaneously 40 times in a minute. The thermometer in the rectum had fallen to 90°.

The air being examined, was found to contain $\frac{1}{18}$ of carbonic acid.

The rabbit occupied the space of 47 cubic inches.

$\frac{502+52+2-47}{18}=\frac{509}{18}=28{\cdot}275$ cubic inches of carbonic acid evolved in 30 minutes, which is at the rate of 56·55 cubic inches in an hour.

The animal lay as if in a state of profound sleep. At the end of two hours and 20 minutes, from the time of the poison being applied, the thermometer in the rectum had fallen to 79°, and he was again apparently dead; but the heart still continued acting, though feebly, and its action was kept up for 30 minutes

longer by means of artificial breathing, when the thermometer had fallen to 76°. The carbonic acid evolved during these last 30 minutes amounted to nearly 13 cubic inches.

From the precautions with which these experiments were made, there is reason to believe that there can be no material error in their results. They appear to warrant the conclusion, that in an animal in which the brain has ceased to exercise its functions, although respiration continues to be performed, and the circulation of the blood is kept up to the natural standard; although the usual changes in the sensible qualities of the blood take place in the two capillary systems, and the same quantity of carbonic acid is formed as under ordinary circumstances, no heat is generated, and that (in consequence of the cold air thrown into the lungs) the animal cools even more rapidly than one which is actually dead.

It is a circumstance deserving of notice, that so large a quantity of air should be consumed by the blood passing through the lungs, when the functions of the brain, and those of the organs dependent on it, are suspended. Perhaps it is not unreasonable to suppose, that by pursuing this line of investigation, we may be enabled to arrive at some more precise knowledge respecting the nature of respiration, and the purposes which it answers in the animal economy. It would however be foreign to the plan of the present communication, to enter into any speculations on this subject, and I shall therefore

only remark, that the influence of the nervous system does not appear to be necessary to the production of the chemical changes which the blood undergoes in consequence of exposure to the air in the lungs.*

The facts now, as well as those formerly ad-

* This conclusion is directly contrary to that deduced by M. Dupuytren, from a series of experiments, made with a view to ascertain the effects which follow the division of the nerves of the *par vagum*, and it is an object of some importance in the present investigation, to ascertain in what manner the apparently opposite facts, observed by M. Dupuytren and myself, are to be reconciled with each other.

It was observed by this physiologist, that in an animal, in which both the nerves of the *par vagum* are divided, the blood returned from the lungs has a darker colour than natural, and that the animals, on whom this operation is performed, die sooner or later with symptoms of asphyxia, notwithstanding that the air continues to enter the lungs; and hence he concludes, that the changes which are produced on the blood in respiration, are not the result of a mere chemical process, but are dependent on the nervous influence, so that they cease to take place when the communication between the lungs and the brain is destroyed.

M. Provençal, in prosecuting this inquiry, ascertained that the animals subjected to this experiment, give out less carbonic acid than under ordinary circumstances.

M. Blainville observed, that the frequency of the inspirations is much diminished; and M. Dumas restored the scarlet colour of the arterial blood by artificially inflating the lungs; and from these and other circumstances, he has arrived at conclusions very different from those of M. Dupuytren.

My own observations exactly correspond with those of MM. Dumas and Blainville. After the nerves of the *par vagum* are divided, a less quantity of carbonic acid is evolved, the inspirations are much diminished in frequency, and the blood in the arteries of the general system assumes a darker

duced, go far towards proving, that the temperature of warm-blooded animals is considerably under the influence of thenervous system. But what is the nature of the connection between them? whether is the brain directly or indirectly necessary to the production of heat? These are questions to which no answers can be given, except such as are purely hypothetical. At present we must be content with the knowledge of the insulated fact: future observations may, perhaps, enable us to refer it to some more general principle.

We have evidence, that when the brain ceases to exercise its functions, although those of the heart and lungs continue to be performed, the animal loses the power of generating heat. It would, however, be absurd to argue from this fact, that the chemical changes of the blood in the lungs are in no way necessary to the production of heat, since we know, not only that there is no instance in which it continues to take place after respiration has ceased, but that respiration is necessary to all the vital functions.

hue; but its natural colour may be restored by artificially inflating the lungs, so as to furnish a greater supply of air to the blood circulating through them.

We may suppose that, on the division of these nerves, the sympathy between the lungs and the nerves of inspiration is either very much impaired, or altogether destroyed, so that the animal does not experience the same impulse to draw in fresh air as under ordinary circumstances. In consequence his inspirations become less frequent than natural, and hence arise the phenomena produced by this operation.

It must be owned, that this part of physiology still presents an ample field for investigation.

Of opinions sanctioned by the names of Black, Laplace, Lavoisier, and Crawford, it is proper to speak with caution and respect; but without trespassing on these feelings, I may be allowed to say, that it does not appear to me that any of the theories hitherto proposed, afford a very satisfactory explanation of the source of animal heat.

Where so many and such various chemical processes are going on, as in the living body, are we justified in selecting any one of these for the purpose of explaining the production of heat?

To the general theory of Dr. Black, there is this unanswerable objection, that the temperature of the lungs is not greater than that of the rest of the system. To this objection the ingenious and beautiful theory of Dr. Crawford is not open; but still it is founded on the same basis with that of Dr. Black, "the conversion of oxygen into carbonic acid in the lungs;" and hence it appears to be difficult to reconcile either of them with the results of the experiments which have been related.

It may, perhaps, be urged, that as in these experiments the secretions had nearly, if not entirely, ceased, it is probable that the other changes which take place in the capillary vessels had ceased also; and that, although the action of the air on the blood might have been the same as under ordinary circumstances, there might

not have been the same alteration in the specific heat of this fluid, as it flowed from the arteries into the veins. But on this supposition, if the theory of Dr. Crawford be admitted as correct, there must have been a gradual but enormous accumulation of latent heat in the blood, which we cannot suppose to have taken place without its nature having been entirely altered. If the blood undergoes the usual change in the capillary system of the pulmonary, it is probable that it must undergo the usual change in the capillary system of the greater circulation also, since these changes are obviously dependent on and connected with each other. The blood in the aorta and pulmonary veins was not more florid, and that in the vena cava and pulmonary artery was not less dark-coloured, than under ordinary circumstances. We may moreover remark, that the most copious secretions in the whole body are those of the insensible perspiration from the skin, and of the watery vapour from the mouth and fauces, and the effect of these must, be not to raise, but to lower the animal temperature. Under other circumstances also, the diminution of the secretions is not observed to be attended with a diminution of heat. On the contrary, in the hot fit of a fever, when the scanty dark-coloured urine, dry skin, and parched mouth, indicate that scarcely any secretions are taking place, the temperature of the body is raised above the natural standard, to which it falls when the

constitution returns to its natural state, and the secretions are restored.

It has been observed, by a distinguished chemist, that "the experiments to determine the specific heat of the blood are of so very delicate a nature, that it is difficult to receive them with perfect confidence." * The experiments of Dr. Crawford for this purpose were necessarily made on blood out of the body, and at rest. Now, when blood is taken from the vessels, it immediately undergoes a remarkable chemical change, separating into a solid and a fluid part. This separation is not complete for some time; but whoever takes the pains to make observations on the subject, can hardly doubt that it begins to take place immediately on the blood being drawn. Can experiments on the blood, under these circumstances, lead to any very satisfactory conclusions, respecting the specific heat of blood circulating in the vessels of the body? The diluting the blood with large quantities of water, as proposed by Dr. Crawford, does not altogether remove the objection; for this only retards, it does not prevent, coagulation; and some time must, at any rate, elapse, while the blood is flowing, and the quantity is being measured, during which, the separation of its solid and fluid parts will have begun to take place.

More might be said on this subject; but I feel anxious to avoid, as much as possible, controver-

* Thomson's History of the Royal Society, p. 129.

sial discussion. It is my wish not to advance opinions, but simply to state some facts which I have met with in the course of my physiological investigations. These facts, I am willing to hope, possess some value; and they may perhaps lead to the developement of other facts of much greater importance. Physiology is yet in its infant state. It embraces a great number and variety of phænomena, and of these it is very difficult to obtain an accurate and satisfactory knowledge; but it is not unreasonable to expect, that by the successive labours of individuals, and the faithful register of their observations, it may at last be enabled to assume the form of a more perfect science. *

* See Additional Note A.

III.

Experiments and Observations on the different Modes in which Death is produced by certain Vegetable Poisons.

[From the Philosophical Transactions for 1811, Part I.]

I. The following experiments were instituted with a view to ascertain, in what manner certain substances act on the animal system, so as to occasion death, independently of mechanical injury. I was led to the inquiry, from the subject of it appearing to be of considerable interest and importance, and from a hope, that, in the present improved state of physiological knowledge, we might be enabled to arrive at some more satisfactory conclusions than had been deduced from any former observations.

The substances which act as poisons when applied to the animal body are very numerous. In the experiments which I have hitherto made, I have employed vegetable poisons only. Of these I have selected such as are very active and certain in producing their effects, believing that, on this account, the exact nature of those effects would be more readily ascertained. The principal objects which I have kept in view have been to determine, on which of the vital organs the

poison employed exercises its primary influence, and through what medium that organ becomes affected. I have also endeavoured to ascertain by what means the fatal consequences of some poisons may be prevented. With some of the conclusions which I have ventured to draw, as far as I know, we were not before acquainted; and others of them, though not entirely new, had not been previously established by satisfactory experiments.

I shall relate first those experiments in which poisons were applied internally, that is, to the mucous membranes of the tongue or alimentary canal, and afterwards those in which poisons were applied to wounded surfaces.

II. *Experiments with Poisons applied to the Tongue, or Alimentary Canal.*

Experiments with Alcohol.

When spirits are taken into the stomach, in a certain quantity, they produce, not at once, but gradually, that kind of delirium which constitutes intoxication: when taken in a larger quantity, it is well known that they destroy life altogether, and that in the course of a very short space of time. Intoxication is a derangement of the functions of the mind, and, as these are in some way connected with those of the brain, it seems probable, that it is by acting on this organ, that spirits, when taken into the stomach, occasion

death. In order to ascertain how far this conclusion is just, I made the following experiments. *

EXPERIMENT I.

I poured two drachms of proof spirits down the œsophagus of a cat. Instantly he struggled violently; then lay on one side, perfectly motionless and insensible; the breathing was laboured and stertorous, and the pulsations of the heart were very frequent. He continued in this state for seven or eight minutes; then began to recover; the respirations became easier, and presently he stood up, and was able to walk.

EXPERIMENT II.

I injected an ounce and a half of proof spirits into the stomach of a large full-grown rabbit, by means of an elastic gum tube passed down the œsophagus. The same symptoms took place as in the last experiment; but the animal did not begin to recover from the state of insensibility until 40 minutes had elapsed from the time of the injection.

EXPERIMENT III.

Seven drachms of proof spirits were injected into the stomach of a younger rabbit. Two

* I am indebted to Dr. E. N. Bancroft for his assistance in many of the experiments which I am about to detail. Mr. W. Brande lent me his assistance in the greater part of those which were made. I have been further assisted on these, as well as on former occasions, by Mr. Broughton, and by some of my own pupils.

minutes afterwards he evidently was affected by the spirits, and in three minutes more he lay on one side motionless and insensible. The pupils of the eyes were perfectly dilated; there were occasional slight convulsive motions of the extremities; the respiration was laborious, it was gradually performed at longer and longer intervals, and at the end of an hour and fifteen minutes had entirely ceased. Two minutes after the animal was apparently dead, I opened into the thorax, and found the heart acting with moderate force and frequency, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration by inflating the lungs, and found that by these means the action of the heart might be kept up to the natural standard, as in an animal from whom the head is removed.

EXPERIMENT IV.

I injected into the stomach of a rabbit two ounces of proof spirits. The injection was scarcely completed, when the animal became perfectly insensible. Precisely the same symptoms took place as in the last experiment, and at the end of twenty-seven minutes from the time of the injection, the rabbit was apparently dead; but on examining the thorax the heart was found still acting, as in the last experiment.

It has been shown by M. Bichat, and the observation has been confirmed by some experiments which I have lately had the honour of

communicating to this learned Society, that the brain is not directly necessary to the action of the heart, and that, when the functions of the brain are destroyed, the heart continues to contract for some time afterwards, and then ceases only in consequence of the suspension of respiration, which is under the influence of the brain.

It would appear, from the experiments which I have just detailed, that the symptoms produced by a large quantity of spirits taken into the stomach, arise entirely from disturbance of the functions of the brain. The complete insensibility to external impressions; the dilatation of the pupils of the eyes; and the loss of motion, indicate that the functions of this organ are suspended; respiration, which is under its influence, is ill performed, and at last altogether ceases; while the heart, to the action of which the brain is not directly necessary, continues to contract, circulating dark-coloured blood for some time afterwards.

There is a striking analogy between the symptoms arising from spirits taken internally, and those produced by injuries of the brain.

Concussion of the brain, which may be considered as the slightest degree of injury, occasions a state of mind resembling intoxication, and the resemblance in some instances is so complete, that the most accurate observer cannot form a diagnosis, except from the history of the case. Pressure on the brain, which is a more severe

injury than concussion, produces loss of motion, insensibility, dilatation of the pupils; the respiration becomes laboured and stertorous, is performed at long intervals, at last altogether ceases, and the patient dies.

It forms an interesting matter of inquiry, whether spirits when taken into the stomach produce their effects on the brain, by being absorbed into the circulation, or in consequence of the sympathy that exists between these organs by means of the nerves. The following circumstances lead me to conclude that, when taken in large quantity so as to produce an immediate effect, they act in the last of these two ways.

1. In experiments where animals have been killed by the injection of spirits into the stomach, I have found this organ to bear the marks of great inflammation, but never found any preternatural appearances whatever in the brain. 2. The effects of spirits taken into the stomach in the last experiment were so instantaneous, that it is difficult to suppose that absorption should have taken place before they were produced. 3. A person who is intoxicated, frequently becomes suddenly sober after vomiting, 4. In the experiments which I have just related, I mixed tincture of rhubarb with the spirits, knowing from the experiments of Mr. Home and Mr. William Brande, that this, when absorbed into the circulation, was readily separated by the kidneys, and that very small quantities might be detected in the urine by the addition of potash;

but, though I never failed to find urine in the bladder, I never detected rhubarb in it.

The including the termination of the thoracic duct in a ligature does not prevent spirits, when taken into the stomach, from producing their usual effects on the nervous system: but subsequent observations, which Mr. Home has already communicated to this Society, have shown that no conclusion can be drawn from this experiment.

That a poison may affect a distant organ, through the medium of the nerves, without entering the circulation, is proved by the well-known circumstance of a solution of the extract of *belladonna*, when applied to the tunica conjunctiva of the eye, occasioning dilatation of the pupil of the same eye, though no other part of the system is affected.

It has been formerly supposed by Dr. Mead and other physiologists, that a poison may produce death by acting on the extremities of the nerves of the stomach and intestines, without being absorbed into the circulation. That it should by these means be capable of affecting the brain is in no degree remarkable, considering the numerous and various sympathies between this organ and the alimentary canal, evidently independent of any other communication than the nerves. *

* See Additional Note B.

Experiments with the Essential Oil of Bitter Almonds.

EXPERIMENT V.

One drop of the essential oil of bitter almonds was applied to the tongue of a young cat. She was instantly seized with violent convulsions; then lay on one side motionless, insensible, breathing in a hurried manner; the respiration became laborious, took place at longer and longer intervals, and at the end of five minutes, from the application of the poison, had entirely ceased, so that the animal was apparently dead; but, on opening the thorax, the heart was found acting regularly 80 times in a minute, circulating dark-coloured blood, and it continued to act for six or seven minutes afterwards.

EXPERIMENT VI.

I injected into the rectum of a cat half an ounce of water, with two drops of the essential oil. In two minutes afterwards, he was affected with symptoms similar to those which occurred in the last experiment, and at the end of five minutes, from the injection of the poison, he was

* The essential oil of bitter almonds does not appear to differ from the essential oil of laurel. I was furnished with a quantity of it, first by my friend Mr. William Brande, and afterwards by Mr. Cooke, of Southampton Street.

apparently dead. Two minutes afterwards the heart was found acting 80 times in a minute. On dissection, no preternatural appearances were found either in the internal membrane of the rectum, or in the brain.

The symptoms produced by this poison, and the circumstance of the heart continuing to contract after apparent death, lead to the conclusion that it occasions death by disturbing the functions of the brain.

While engaged in these last experiments, I dipped the blunt end of a probe into the essential oil, and applied it to my tongue, meaning to taste it, and having no suspicion that so small a quantity could produce any of its specific effects on the nervous system; but scarcely had I applied it, when I experienced a very remarkable and unpleasant sensation, which I referred chiefly to the epigastric region, but the exact nature of which I cannot describe, because I know nothing precisely similar to it. At the same time there was a sense of weakness in my limbs, as if I had not the command of my muscles, and I thought that I was about to fall. However, these sensations were momentary, and I experienced no inconvenience whatever afterwards.

I afterwards applied a more minute quantity of the essential oil to my tongne several times, without experiencing from it any disagreeable effects; but on applying a larger quantity, I was affected with the same momentary sensations as in the former instance, and there was a recur-

rence of them in three or four seconds after the first attack had subsided.*

From the instantaneousness with which the effects are produced; and from its acting more speedily when applied to the tongue than when injected into the intestine, though the latter presents a better absorbing surface, we may conclude that this poison acts on the brain through the medium of the nerves, without being absorbed into the circulation.

Experiment with the expressed Juice of the Leaves of Aconite.

EXPERIMENT VII.

An ounce of this juice was injected into the rectum of a cat. Three minutes afterwards he voided what appeared to be nearly the whole of the injection; he then stood for some minutes perfectly motionless, with his legs drawn together. At the end of nine minutes, from the time of the injection, he retched and vomited; then attempted to walk, but faltered and fell at every step, as if from giddiness. At the end of thirteen minutes, he lay on one side insensible, motionless, except some slight convulsive motions of the limbs. The respiration was slow and laboured; and at 47 minutes from the time of the injection, he was apparently dead. One minute and a half afterwards the heart was

* See Additional Note C.

found contracting regularly one hundred times in a minute.

It appears from this experiment, that the juice of aconite, when injected into the intestine, occasions death by destroying the functions of the brain. From the analogy of other poisons, it is rendered probable that it is capable of influencing the brain through the medium of the nerves, without being absorbed into the circulation. This opinion is confirmed by the following circumstance: if a small quantity of the leaf of aconite is chewed, it occasions a remarkable sense of numbness of the lips and gums, which does not subside for two or three hours.

Experiments with the Infusion of Tobacco.

EXPERIMENT VIII.

Four ounces of infusion of tobacco were injected into the rectum of a dog. Four minutes afterwards he retched, but did not vomit; he then became faint, and lay motionless on one side; at the end of nine minutes from the time of the injection, the heart could not be felt; he gasped for breath at long intervals; and in another minute there was no appearance whatever of life. I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines.

EXPERIMENT IX.

An ounce of very strong infusion of tobacco was injected into the rectum of a cat. Symptoms were produced similar to those which occurred in the last experiment, and the animal died at the end of seven minutes from the time of the injection. On opening the thorax immediately after death, the heart was found extremely distended, and to have entirely ceased acting, with the exception of a slight tremulous motion of the auricles.

EXPERIMENT X.

Three ounces of infusion of tobacco were injected into the rectum of a dog. He was affected with symptoms similar to those in the former experiments, and died at the end of 10 minutes. On opening the thorax immediately after death, I found the heart much distended, and to have entirely ceased contracting.

EXPERIMENT XI.

Three ounces of infusion of tobacco were injected into the rectum of a dog. Immediately there took place tremulous contractions of the voluntary muscles. Five minutes afterwards the injection was repeated in the same quantity. The dog then was sick and threw up some of the infusion, with other matter, from the stomach; he became faint, and died 10 minutes after the second injection. Immediately after respiration had ceased, I opened the thorax, and found the

heart extremely distended, and without any evident contraction, except of the appendix of the right auricle, which every now and then contracted in a slight degree. I divided the pericardium on the right side. In consequence of the extreme distension of the heart, this could not be done without irritating the fibres with the point of the scalpel. Immediately both auricles and ventricles began to contract with considerable force, so as to restore the circulation. Artificial respiration was produced, and the circulation was kept up for more than half an hour, beyond which time the experiment was not continued.

We may conclude from these experiments, that the effect of the infusion of tobacco, when injected into the intestine of a living animal, is to destroy the action of the heart, stopping the circulation and producing syncope.* It appeared to me that the action of the heart ceased even before the animal had ceased to respire; and this was confirmed by another experiment, in which, in a dog killed by the infusion of tobacco, I found the cavities of the left side of the heart to contain scarlet blood, while in those of the right side the blood was dark-coloured. This poison, therefore, differs materially from alcohol, the essential oil of almonds, and the juice of aconite, which have no direct influence on the action of the heart. The infusion of

* See Additional Note D.

tobacco renders the heart insensible to the accustomed stimulus, but it does not altogether destroy the power of muscular contraction, since the heart resumed its action in one instance on the division of the pericardium; and I have found that the voluntary muscles of an animal killed by this poison, are as readily stimulated to contract by the influence of the Voltaic battery, as if it had been killed in any other manner. At the same time, however, that the infusion of tobacco destroys the action of the heart, it appears to destroy also the functions of the brain, since these did not return in the last experiment; although the circulation was restored, and kept up by artificial respiration.

There being no direct communication between the intestine and the heart, I was at first induced to suppose that the latter becomes affected only in consequence of the infusion being conveyed into the blood. Some circumstances in the following experiment have since led me to doubt whether it may not exercise an influence over the system independently of absorption.

EXPERIMENT XII.

In a dog, whose head was removed, I kept up the circulation by means of artificial respiration, in the manner already described in a former communication to this Society. I then injected into the stomach and intestines nine ounces of infusion of tobacco. At the time of the injection, the body

of the animal lay perfectly quiet and motionless on the table; the heart acted regularly 100 times in a minute. Ten minutes afterwards the pulse rose to 140 in a minute; the peristaltic motion of the intestines was much increased, and the voluntary muscles in every part of the body were thrown into repeated and violent spasmodic action. The joints of the extremities were alternately bent and extended; the muscles of the spine, abdomen, and tail alternately relaxed and contracted, so as to turn the whole animal from one side to the other. I have observed, in other instances, spasmodic actions of the muscles, where the circulation was kept up by artificial respiration, after the removal of the head, but not at all to be compared, either in strength or frequency, with those which took place on this occasion. I made pressure on the abdominal aorta for more than a minute, so as to obstruct the circulation of the blood in the lower extremities; but the muscular contractions were not lessened in consequence. Half an hour after the injection of the infusion, the artificial respiration was discontinued. The heart continued to act, circulating dark-coloured blood; and the muscular contractions continued, but gradually diminishing in strength and frequency. I tied a ligature round the vessels at the base of the heart, so as to stop the circulation; nevertheless the muscular contractions continued, though less frequent and forcible than before,

and some minutes elapsed before they had entirely ceased.

In this experiment, the disposition to contraction in the muscles was very much increased, instead of being diminished, as in those before related. If the infusion of tobacco influences the heart merely from being absorbed into the blood, and thus coming into actual contact with its fibres, there is no evident reason why the removal of the brain, and the employment of artificial respiration, should occasion so material a difference in its effects. If the contractions of the voluntary muscles had depended on the infusion circulating with the blood, it is reasonable to suppose that the pressure on the aorta would have occasioned some diminution of them, and that the complete obstruction of the circulation would have caused them to cease altogether.

From these considerations, I am induced to believe that the infusion of tobacco, when injected into the intestines, influences the heart through the medium of the nervous system; but I have not been able to devise any experiment by which the truth or fallacy of this opinion might be put beyond the reach of doubt.

It appears remarkable, that the brain and nervous system, although not necessary to the action of the heart, should, when under the influence of the infusion of tobacco, be capable of acting on this organ so as to stop its action; but this is analogous to what occurs in consequence of violent emotions of the mind. Those

states of the nervous system which accompany the passions of joy, fear, or anger, when existing in a moderate degree, have the effect of increasing the frequency of the contractions of the heart, and the rapidity of the circulation, while the same passions, if they exist in a greater degree, produce the contrary effect, so that syncope ensues.

Experiments with the Empyreumatic Oil of Tobacco.*

EXPERIMENT XIII.

Less than a drop of this oil was applied to the tongue of a young cat. Instantly violent convulsions took place in all the muscles, and the respirations became very frequent. In five minutes after the application, she lay on one side insensible, with slight spasmodic actions of the muscles. At the end of 11 minutes she retched, but did not vomit. In a quarter of an hour she appeared to be recovering. I repeated the application of the poison, and she was again seized with violent convulsions, and became insensible, breathing at long intervals, and in two

* I was furnished with the empyreumatic oil of tobacco by Mr. W. Brande. It may be produced by subjecting the leaves of tobacco to distillation in a heat above that of boiling water. A watery fluid comes over, on the surface of which is the oil appearing as a thin film of unctuous substance.

minutes from the second application respiration had entirely ceased, and she was apparently dead. On opening the thorax, I found the heart acting with regularity and strength, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration. The contractions of the heart became augmented in force and frequency, and there was no evident diminution in six or seven minutes, during which the artificial respiration was continued.

On dissection, nothing remarkable was found in the appearance of the tongue or brain.

The symptoms and mode of death in this experiment did not essentially differ from those produced by the essential oil of almonds. I was surprised to find the effects of the empyreumatic oil so entirely different from those of the infusion of tobacco. Supposing that this difference might arise from the poison being more concentrated in the oil than in the infusion, I made the following experiments.

EXPERIMENT XIV.

A drop of the oil of tobacco was suspended in an ounce and a half of water, by means of mucilage of gum arabic, and the whole was injected into the rectum of a dog. In two minutes afterwards he became faint, retched, but did not vomit. He appeared to be recovering from this state, and in 25 minutes after the first injection, it was repeated in the same quantity. He was

then seized with symptoms similar to those in the last experiment, and in two minutes and a half he was apparently dead.

Two minutes after apparent death, on the thorax being opened into, the heart was found acting regularly 100 times in a minute, and it continued acting for several minutes.

EXPERIMENT XV.

A drop of the empyreumatic oil of tobacco with an ounce of water was injected into the rectum of a cat. The symptoms produced were, in essential circumstances, similar to those which occurred in the last experiment. The animal was apparently dead in five minutes after the injection, and the heart continued to contract for several minutes afterwards.

We may conclude from these experiments, that the empyreumatic oil of tobacco, whether applied to the tongue, or injected into the intestine, does not stop the action of the heart and induce syncope, like the infusion of tobacco ; but that it occasions death by destroying the functions of the brain, without directly acting on the circulation. In other words, its effects are similar to those of alcohol, the juice of aconite, and the essential oil of almonds.

III. *Experiments with Poisons applied to Wounded Surfaces.*

Experiments with the Essential Oil of Almonds.

EXPERIMENT XVI.

I made an incision in the thigh of a rabbit, and introduced two drops of the essential oil of almonds between the skin and the muscles. In four minutes after the application, he was seized with violent convulsions, and became insensible, and in two minutes more he was apparently dead; but the heart was felt through the ribs acting 120 times in a minute, and it continued acting for several minutes. There were no other appearances in the limb, than would have resulted from an ordinary wound.

EXPERIMENT XVII.

Two drops of the essential oil of almonds were introduced into a wound in the side of a mouse. Two minutes afterwards he was affected with symptoms similar to those which occurred in the last experiment, and in two minutes more he was apparently dead, but the heart continued to contract for some minutes afterwards.

From the experiments which I have just related, and from others which it appears unnecessary to detail, as the general results were the

same, I have learned that where the essential oil of almonds is applied to a wound, its effects are *not so instantaneous* as when it is applied to the tongue; but that otherwise there is no difference in its effects in whichever manner it is used.

Experiments with the Juice of the Leaves of the Aconite.

EXPERIMENT XVIII.

I made a wound in the side of a young rabbit, and introduced, between the skin and muscles, about twenty drops of the juice of aconite. 23 minutes afterwards he was affected with symptoms, in all essential respects similar to those which occurred in an experiment already related, where the juice was injected into the rectum, and at the end of 47 minutes from the application of the poison, he was apparently dead. Two minutes after apparent death, the heart was found contracting, but very feebly.

Experiments with the Woorara.*

EXPERIMENT XIX.

A small quantity of the woorara in powder was applied to a wound in the side of a Guinea

* The Woorara is a poison with which the Indians of Guiana arm the points of their arrows. It appears not to

pig. In 10 minutes afterwards the animal was unable to walk; then he became quite motionless, except some slight occasional convulsions. He gradually became insensible, the respirations were laboured, and at the end of 14 minutes from the application of the poison, the respiration had entirely ceased, and he was apparently dead; but on opening the thorax, the heart was found acting 70 times in a minute, circulating dark-coloured blood, and it continued to contract for several minutes afterwards. On dissection, no preternatural appearances were observed in the brain; nor was there any other appearance in the limb than would have arisen from an ordinary wound.

EXPERIMENT XX.

I made a wound in the side of a Guinea pig, and introduced into it about two grains of the woorara in powder. At the end of 25 minutes, symptoms took place very similar to those which occurred in the last experiment, and in 13 minutes more the animal was apparently dead; but the heart continued to contract 108 times in a minute, and by means of artificial respiration the circulation was kept up for more than 20 minutes.

differ essentially from the Ticunas, which was employed in the experiments of the Abbé Fontana. I am indebted to Dr. E. N. Bancroft, who not only furnished me with some of the Woorara which he had in his possession, but also lent me his assistance in the experiments which were made with it.

The results of other experiments which I have made with the woorara, were similar to those just described. The heart continued to act after apparent death, and the circulation might be kept up by means of artificial respiration. It is evident that this poison acts in some way or another on the brain, and that the cessation of the functions of this organ is the immediate cause of death.

I found in these experiments, that the best mode of applying the woorara is by dissolving it in water to the consistence of a thin paste. I first made the wound, and then smeared the poison over it with the end of the scalpel. It seemed that the animal was more speedily and certainly affected, if there was some hæmorrhage, unless the hæmorrhage was very copious, when it produced an opposite effect, by washing the poison away from the wound. When the poison was applied in a large quantity, it sometimes began to act in six or seven minutes. Never more than half an hour elapsed from the time of the poison being inserted, to that of the animal being affected, except in one instance where a ligature was applied on the limb, as will be explained afterwards. The woorara which I employed had been preserved for some years, which will account for its having been less active than it has been described to be, by those who had witnessed its effects when in a recent state.*

* See Additional Note E.

Experiments with the Upas Antiar.*

EXPERIMENT XXI.

About two grains of this poison were made into a thin paste with water, and inserted into a wound in the thigh of a dog. Twelve minutes afterwards he became languid; at the end of 15 minutes, the heart was found to beat very irregularly, and with frequent intermissions; after which, he had a slight rigor. At the end of 20 minutes the heart beat very feebly and irregularly; the dog was languid, sick, and vomited; but the respirations were as frequent and as full as under natural circumstances, and he was perfectly sensible. At the end of 20 minutes he suddenly fell on one side, and was apparently dead. I immediately opened the thorax, and found the heart distended with blood in a very remarkable degree, and to have entirely ceased contracting. There was one distinct and full inspiration after I had begun making the incision into the thorax. The cavities of the left side of the heart contained scarlet blood, and those of the right side contained dark-coloured blood, as in a living animal.

* We are informed that the island of Java produces two powerful vegetable poisons, to one of which the natives give the name of *Upas tieutè*, and to the other that of *Upas antiar*. I was supplied with a quantity of the latter through the kindness of Mr. Marsden, who had some of it in his possession.

EXPERIMENT XXII.

A small quantity of the upas antiar, prepared as before, was inserted into a wound in the thigh of a young cat. She appeared languid in two minutes after the poison was inserted. The symptoms which took place did not essentially differ from those which occurred in the last experiment, except that there were some convulsive motions of the limbs. At eight minutes after the poison was inserted she lay on one side, motionless and insensible; the heart could not be felt, but the respiration had not entirely ceased. On opening the thorax, I found the heart to have ceased contracting. It was much distended with blood: and the blood in the cavities of the left side was of a scarlet colour. There were two full inspirations after the incision of the thorax was begun. On irritating the heart with the point of the scalpel, slight contractions took place in the fibres of the appendices of the auricles, but none in any other part.

EXPERIMENT XXIII.

The experiment was repeated on a rabbit. The symptoms produced were similar to those in the last experiment; but the animal did not vomit, and the convulsive motions were less in degree. He died 11 minutes after the poison was

inserted. On opening the thorax, the heart was found to have entirely ceased contracting. It was much distended with blood; and the blood in the cavities of the left side was of a scarlet colour. On irritating the heart with the point of the scalpel, the ventricles contracted, but not sufficiently to restore the circulation.

EXPERIMENT XXIV.

About a grain of the upas antiar was inserted into a wound in the side of a rabbit. He was affected with symptoms similar to those before described, and died in 10 minutes after the poison was applied. On opening the thorax immediately after death, the heart was found to have ceased contracting, and the blood in the cavities of the left side was of a scarlet colour.

It appears, from these experiments, that the upas antiar, when inserted into a wound, produces death (as infusion of tobacco does when injected into the intestine) by rendering the heart insensible to the stimulus on which its action depends, and stopping the circulation. The heart beats feebly and irregularly before either the functions of the brain, or the respiration appear to be affected. Respiration is performed even after the circulation has ceased; and the left side of the heart is found after death to contain scarlet blood, which never can be the case, where the cause of death is the cessation of the functions of

the brain or lungs. The convulsions, which occur when the circulation has nearly ceased, probably arise from the diminution of the supply of blood to the brain, resembling those which accompany death from hæmorrhage.

There remains an interesting subject of inquiry, "through what medium do poisons influence the brain when applied to wounds?" That poisons applied in this manner do not produce their effects precisely in the same way as poisons taken internally, is rendered probable by this circumstance; that some poisons, which are very powerful when applied to wounds even in small quantities, are either altogether inefficient when taken internally, or require to be given in very large quantities, in order to produce their effect, and *vice versâ*.

A poison applied to a wounded surface may be supposed to act on the brain in one of three ways:

1. By means of the nerves, like poisons taken internally.

2. By passing into the circulation through the lymphatic vessels.

3. By passing directly into the circulation through the divided veins.

EXPERIMENT XXV.

In order to ascertain whether the woorara acts through the medium of the nerves, I exposed the axilla of a rabbit, and divided the

spinal nerves supplying the upper extremity, just before they unite to form the axillary plexus. I not only divided every nervous filament, however small, which I could detect, but every portion of cellular membrane in the axilla, so that the artery and vein were left entirely insulated. I then made two wounds in the forearm, and inserted into them some of the woorara formed into a paste. Fourteen minutes after the poison was applied, the hind legs became paralytic, and in 10 minutes more the animal died, with symptoms precisely similar to those which took place in the former experiments with the same poison, the heart continuing to act after apparent death. On dissection, the nerves of the upper extremity were very carefully examined, but not the smallest filament could be found undivided.

I made the following experiment to ascertain whether the woorara passes into the circulation through the lymphatic vessels.

EXPERIMENT XXVI.

I tied a ligature round the thoracic duct of a dog, just before it perforates the angle of the left subclavian and jugular veins. I then made two wounds in the left hind leg, and introduced some of the woorara in powder into them. In less than a quarter of an hour the animal became affected with the usual symptoms, and died in a few minutes afterwards.

After death, I dissected the thoracic duct with great care, and found it to have been perfectly secured by the ligature. It was very much distended with chyle, and about two inches below its termination its tunicks had given way, and chyle was extravasated into the cellular membrane. The lymphatic vessels in the left axilla were distended in a very remarkable degree, and on dividing them, not less than a drachm of lymph issued from the divided ends.

Since neither the division of the nerves, nor the obstruction of the thoracic duct interfere in the slightest degree with the effects of the woorara, there was presumptive evidence that it acts on the brain by entering the circulation through the divided veins. I next endeavoured to ascertain, by a more direct experiment, whether this is really the case.

To apply ligatures to the large vessels of a limb only, would evidently lead to no satisfactory conclusion, since the anastomosing vessels might still carry on the circulation. The only method, which I could devise, of obtaining the desired information, was to include all the vessels, small as well as large, in a ligature.

EXPERIMENT XXVII.

In order to make the experiment more satisfactorily, I exposed the sciatic nerve of a rabbit in the upper and posterior part of the thigh, and passed under it a tape half an inch wide. I then

made a wound in the leg, and having introduced into it some of the woorara mixed with water, I tied the tape moderately tight on the fore part of the thigh. Thus I interrupted the communication between the wound and the other parts of the body, by means of the vessels, while that by means of the nerve still remained. After the ligature was tightened, I applied the woorara a second time, in another part of the leg. The rabbit was not at all affected, and at the end of an hour I removed the ligature. Being engaged in some other pursuit, I did not watch the animal so closely as I should otherwise have done; but 20 minutes after the ligature had been removed, I found him lying on one side, motionless and insensible, evidently under the influence of the poison. The symptoms however were less violent than in most instances; and after some time he recovered, with the limb perfectly warm and using it as well as before.

EXPERIMENT XXVIII.

I repeated the last experiment with this difference, that after having applied the poison, I made the ligature as tight as I could draw it. I removed the ligature at the end of an hour and 20 minutes, but the animal was not at all affected either before or after the removal of the ligature, and on the following day he had recovered the use of the limb.

EXPERIMENT XXIX.

I made the experiment a third time on a rabbit, drawing the ligature very tight. At the end of 45 minutes, he continued perfectly well, and the ligature was removed. I watched him for three quarters of an hour afterwards, but there were no symptoms of his being affected by the poison. On the following day he died, but this I attribute to the mechanical injury done to the limb and sciatic nerve, as there was the appearance of inflammation in the parts in the neighbourhood of the ligature.

These three experiments were made with the greatest care. From the mode in which the poison was applied, from the quantity employed, and from my prior experience, I should have entertained not the smallest doubt of the poison taking effect in every instance in less than 20 minutes, if no ligature had been applied. In two of the three, the quantity of woorara was more than had been used in any former experiments.

I have not judged it necessary to make any more experiments with the ligature on the limb, because the numerous experiments of the Abbé Fontana on the ticunas coincide in their results with those which have just been detailed, and fully establish the efficacy of the ligature, in preventing the action of the poison. It is not to be wondered at, that the ligature should sometimes

fail in its effects, since these must evidently depend on the degree in which the circulation is obstructed, and on the length of time during which the obstruction is continued.

There can then be little doubt that the woorara affects the brain, by passing into the circulation through the divided vessels.* It is probable, that it does not produce its effects, until it enters the substance of the brain, along with the blood, in which it is dissolved; nor will the experiments of the Abbé Fontana, in which he found the ticunas produce almost instant death when injected into the jugular vein of a rabbit, be found to militate against the conclusion, when we consider how short is the distance, which, in so small an animal, the blood has to pass from the jugular vein to the carotid artery, and the great rapidity of the circulation; especially in a rabbit under the influence of powerful excitement, in whom the heart cannot be supposed to act so seldom as three times in a second.

I have made no experiments to ascertain through what medium other poisons operate when applied to wounds; but from analogy, we may suppose, that they also enter the circulation through the divided blood vessels.

IV. The facts already related, led me to conclude that alcohol, the essential oil of almonds, the juice of aconite, the oil of tobacco, and the

* See Additional Note F.

woorara, occasion death simply by destroying the functions of the brain. The following experiment appears fully to establish the truth of this conclusion.

EXPERIMENT XXX.

The temperature of the room being 58° Fahrenheit's thermometer, I made two wounds in the side of a rabbit, and applied to them some of the woorara in the form of paste. In seven minutes after the application, the hind legs were paralysed, and in 15 minutes respiration had ceased, and he was apparently dead. Two minutes afterwards the heart was still beating, and a tube was introduced through an opening into the trachea, by means of which the lungs were inflated. The artificial respiration was made regularly about 36 times in a minute.

At first, the heart contracted 100 times in a minute.

At the end of 40 minutes, the pulse had risen to 120 in a minute.

At the end of an hour, it had risen to 140 in a minute.

At the end of an hour and 23 minutes, the pulse had fallen to 100, and the artificial respiration was discontinued.

At the commencement of the experiment, the ball of a thermometer being placed in the rectum, the quicksilver rose to 100°; at the close of the experiment it had fallen to 88½°

During the continuance of the artificial respiration, the blood in the femoral artery was of a florid red, and that in the femoral vein was of a dark colour, as usual.

It has been observed by M. Bichat, that the immediate cause of death, when it takes place suddenly, must be the cessation of the functions of the heart, the brain, or the lungs. This observation may be extended to death under all circumstances. The stomach, the liver, the kidneys, and many other organs are necessary to life, but their *constant* action is not necessary; and the cessation of their functions cannot therefore be the *immediate* cause of death. As in this case the action of the heart had never ceased; as the circulation of the blood was kept up by artificial respiration for more than an hour and 20 minutes after the poison had produced its full effects; and as during this time the usual changes in the colour of the blood took place in the lungs; it is evident, that the functions of the heart and lungs were unimpaired: but that those of the brain had ceased, is proved, by the animal having continued in a state of complete insensibility, and by this circumstance, that animal heat, to the generation of which I have formerly shown the influence of the brain to be necessary, was not generated.

Having learned that the circulation might be kept up by artificial respiration for a considerable time after the woorara had produced its full effects, it occurred to me, that in an animal under

the influence of this or of any other poison that acts in a similar manner, by continuing the artificial respiration for a sufficient length of time after natural respiration had ceased, the brain might recover from the impression which the poison had produced, so that it would be restored to life. In the last experiment, the rabbit gave no signs of returning sensibility; but it is to be observed, 1. That the quantity of poison employed was very large. 2. That there was a great loss of animal heat, in consequence of the temperature of the room being much below the natural temperature of the animal, which could not therefore be considered under such favourable circumstances as to recovery, as if it had been kept in a higher temperature. 3. That the circulation was still vigorous when I left off inflating the lungs, and therefore it cannot be known what would have been the result, if the artificial respiration had been longer continued.

EXPERIMENT XXXI.

A wound was made in the side of a rabbit. One drop of the essential oil of almonds was inserted into it; and immediately the animal was placed in a temperature of 90°. In two minutes he was under the influence of the poison. The usual symptoms took place. In three minutes more, respiration had ceased, and he lay apparently dead, but the heart was still felt beating through the ribs. A tube was then intro-

duced into one of the nostrils, and the lungs were inflated about 35 times in a minute. Six minutes after the commencement of artificial respiration, he moved his head and legs, and made an effort to breathe. He then was seized with convulsions, and again lay motionless, but continued to make occasional efforts to breathe. Sixteen minutes after its commencement, the artificial respiration was discontinued. He now breathed spontaneously 70 times in a minute, and moved his head and extremities. After this he occasionally rose, and attempted to walk. In the intervals he continued in a dozing state, but from this he gradually recovered. In less than two hours he appeared perfectly well, and he continued well on the following day.

The inflating the lungs has been frequently recommended in cases of suffocation, where the cause of death is the cessation of the functions of the lungs: As far as I know, it has not been before proposed in those cases in which the cause of death is the cessation of the functions of the brain.* It is probable that this method of treat-

* Since this paper was read, I have been favoured by the Right Hon. the President with the perusal of a Dissertation on the Effects of the Upas Tieutè, lately published at Paris by M. Delile, by which I find that he had employed artificial respiration for the purpose of recovering animals which were under the influence of this poison, with success. M. Delile describes the Upas Tieutè as causing death by occasioning repeated and long continued contractions of the muscles of respiration, on which it acts through the medium of the spinal chord, without destroying the functions of the brain.

ment might be employed with advantage for the recovery of persons labouring under the effects of opium, and many other poisons.

V. The experiments which have been detailed lead to the following conclusions.

1. Alcohol, the essential oil of almonds, the juice of aconite, the empyreumatic oil of tobacco, and the woorara, act as poisons by simply destroying the functions of the brain; universal death taking place, because respiration is under the influence of the brain, and ceases when its functions are suspended.

2. The infusion of tobacco when injected into the intestine, and the upas antiar when applied to a wound, have the power of rendering the heart insensible to the stimulus of the blood, thus stopping the circulation; in other words, they occasion death by syncope.

3. There is reason to believe, that the poisons, which in these experiments were applied internally, produce their effects through the medium of the nerves, independently of their being absorbed into the circulation.

4. When the woorara is applied to a wound, it produces its effects on the brain, by entering the circulation through the divided blood-vessels; and from analogy it appears probable that other poisons, when applied to wounds, operate in a similar manner.

5. When an animal is apparently dead from the influence of a poison which acts merely by

destroying the functions of the brain, it may, in some instances at least, be made to recover, if respiration is artificially produced and continued for a certain length of time.

From analogy we might draw some conclusions respecting the mode in which some other vegetable poisons produce their effects on the animal system; but I forbear to enter into any speculative inquiries, as it is my wish, in the present communication, to record such facts only as appear to be established by actual experiment.

IV.

Further Observations and Experiments on the Action of Poisons on the Animal System.

[From the Philosophical Transactions for 1812.]

Since I had the honour of communicating to the Royal Society some Observations on the Action of certain Poisons on the Animal System, I have been engaged in the further prosecution of this inquiry. Besides some additional experiments on vegetable poisons, I have instituted several with a view to explain the effects of some of the more powerful poisons of the mineral kingdom. The former correspond in their results so nearly with those which are already before the public, that, in the present communication, I shall confine myself to those which appear to be of some importance, as they confirm my former conclusions respecting the recovery of animals apparently dead, where the cause of death operates exclusively on the nervous system. In my experiments on mineral poisons, I have found some circumstances wherein their effects differ from those of vegetable poisons; and of these I shall give a more particular account. Whatever may be the value of the observations themselves, the subject must be

allowed to be one that is deserving of investigation, as it does not appear unreasonable to expect that such physiological investigations may hereafter lead to some practical improvements in the healing art. This consideration, I hope, will be regarded as a sufficient apology for my pursuing a mode of inquiry, by means of experiments on brute animals, of which we might question the propriety, if no other purpose were to be answered by it than the gratification of curiosity.

In my former communication on this subject, I entered into a detailed account of the majority of the experiments which were made. This I conceived necessary, because, in the outset of the inquiry, I had been led to expect that even the same poison may not always operate precisely in the same manner. But I have since had abundant proof that, in essential circumstances, there is but little variety in the effects produced by any one poison, when employed on animals of the same, or even of different species, beyond what may be referred to difference in quantity, or the mode of its application, or of the age and power of the animal. This will explain the reason of my not detailing, in the present communication, so many of the individual experiments from which my conclusions are drawn, as in the former. At the same time I have not been less careful to avoid drawing general conclusions from only a limited number of facts. Should these conclusions prove fewer and of less importance than might be expected, such defects

will, I trust, be regarded with indulgence, at least by those who are aware of the difficulty of conducting a series of physiological experiments, of the time which they necessarily occupy, of the numerous sources of fallacy and failure which exist, and of the laborious attention to the minutest circumstances which is in consequence necessary, in order to avoid being led into error.

II. *Experiments with the Woorara.*

In a former experiment, I succeeded in recovering an animal which was apparently dead from the influence of the essential oil of bitter almonds, by continuing respiration artificially until the impression of the poison upon the brain had ceased; while a similar experiment on an animal under the influence of the woorara, was not attended with the same success. Some circumstances led me to believe that the result of the experiment with the woorara might have been different, if it had been made with certain precautions; but I was unable at that time to repeat it, in consequence of my stock of the poison being exhausted. I have since, however, been able to procure a fresh supply; and I shall relate two experiments which I have made with it. In one of these an animal apparently dead from the woorara was made to recover, notwithstanding the functions of the brain appeared to be wholly suspended for a very long period of

time; in the other, though ultimate recovery did not take place, the circulation was maintained for several hours after the brain had ceased to perform its office.

EXPERIMENT I.

Some woorara was inserted into a wound in a young cat. She became affected by it in a few minutes, and lay in a drowsy and half sensible state, in which she continued at the end of an hour and 15 minutes, when the application of the poison was repeated. In four minutes after the second application, respiration had entirely ceased, and the animal appeared to be dead; but the heart was still felt acting about 140 times in a minute. She was placed in a temperature of 85° of Fahrenheit's thermometer, and the lungs were artificially inflated about 40 times in a minute.

The heart continued acting regularly.

When the artificial respiration had been kept up during 40 minutes, the pupils of the eyes were observed to contract and dilate on the increase or diminution of light; saliva had flowed from the mouth, and a small quantity of tears was collected between the eye and eyelids; but the animal still continued perfectly motionless and insensible.

At the end of an hour and 40 minutes from the same period, there were slight involuntary

contractions of the muscles, and every now and then there was an effort to breathe. The involuntary motions continued, and the efforts to breathe became more frequent. At the end of another hour, the animal, for the first time, gave some signs of sensibility when roused, and made spontaneous efforts to breathe 22 times in a minute. The artificial respiration was discontinued. She lay, as if in a state of profound sleep, for 40 minutes more, when she suddenly awoke, and walked away. On the following day she appeared slightly indisposed; but she gradually recovered, and is at this time still alive and in health.

EXPERIMENT II.

Some woorara was applied to a wound in a rabbit. The animal was apparently dead in four minutes after the application of the poison; but the heart continued acting. He was placed in a temperature of 90°, and the lungs were artificially inflated. The heart continued to act about 150 times in a minute. For more than three hours the pulse was strong and regular; after this it became feeble and irregular; and at the end of another hour the circulation had entirely ceased. During this time there was no appearance of returning sensibility.

The circulation of the blood may be maintained in an animal from whom the brain has been removed for a considerable, but not for an

unlimited time. We may conclude that, in the last of these experiments, the animal did not recover, because the influence of the poison continued beyond the time during which the circulation may be maintained without the brain.*

III. *On the Effects of Arsenic.*

When an animal is killed by arsenic taken internally, the stomach is found bearing marks of inflammation; and it is a very general opinion, 1. That this inflammation is the cause of death; 2. That it is the consequence of the actual contact of the arsenic with the internal coat of the stomach. But in several instances I have found the inflammation of the stomach so slight, that on a superficial examination it might have been easily overlooked; and in most of my experiments with this poison, death has taken place in too short a time for it to be considered as the result of inflammation: and hence we may conclude that the first of these opinions is incorrect, at least a general proposition.

Many circumstances conspire to show that the second of these opinions also is unfounded.

In whatever way the poison is administered, the inflammation is confined to the stomach and intestines: I have never seen any appearance of it in the pharynx or œsophagus.

* See Additional Note G.

Mr. Home informed me that, in an experiment made by Mr. Hunter and himself, in which arsenic was applied to a wound in a dog, the animal died in twenty-four hours, and the stomach was found to be considerably inflamed.

I repeated this experiment several times, taking the precaution always of applying a bandage to prevent the animal licking the wound. The result was, that the inflammation of the stomach was commonly more violent and more immediate than when the poison was administered internally, and that it preceded any appearance of inflammation of the wound.* Some experiments are already before the public, which led me to conclude that vegetable poisons, when applied to wounded surfaces, affect the system by passing into the circulation through the divided veins. From this analogy, and from all the circumstances just mentioned, it may be inferred that arsenic, in whatever way it is administered, does not produce its effects even on the stomach until it is carried into the blood.

But the blood is not necessary to life, except so far as a constant supply of it in its healthy

* Since the greater part of my experiments on this subject were made, I have seen an account of an inaugural Dissertation on the Effects of Arsenic, by Dr. Jaëger of Stutgard. Dr. Jaëger has come to conclusions similar to those above stated, that in an animal killed by arsenic, the inflammation of the stomach is not the cause of death, and that the poison does not produce its fatal effects until it has entered the circulation. I have to regret that I have had no opportunity of seeing the original of this dissertation.

state is necessary for the maintenance of the functions of the vital organs. The next object of inquiry therefore is, when arsenic has entered the circulation, on what organs does it operate so as to occasion death?

When arsenic is applied to an ulcerated surface, it produces a slough, not by acting chemically, like caustics in general, but by destroying the vitality of the part to which it is applied, independently of chemical action. This led me at first to suppose that, when arsenic has passed into the circulation, death is the consequence, not so much of the poison disturbing the functions of any particular organ, as of its destroying at once the vitality of every part of the system. The following circumstances, however, seem to show that this opinion is erroneous. In an animal under the full influence of arsenic, even to the instant of death, some of the secretions, as those of the kidneys, stomach, and intestines, continue to take place in large quantity; and the muscles are capable of being excited, after death, to distinct and powerful contractions by means of the Voltaic battery.

EXPERIMENT III.

Seven grains of the white oxyde of arsenic were applied to a wound in the back of a rabbit.

In a few minutes he was languid, and the respirations were small and frequent. The pulse was feeble, and after a little time could not be

felt. The hind legs became paralysed.* He grew insensible, and lay motionless, but with occasional convulsions. At the end of 53 minutes from the time of the arsenic being applied, he was apparently dead; but on opening the thorax, the heart was found still acting, though very slowly and feebly. A tube was introduced into the trachea, and the lungs were artificially inflated; but this appeared to have no effect in prolonging the heart's action. On dissection, the inner membrane of the stomach was found slightly inflamed.

EXPERIMENT IV.

Two drachms of arseniac acid, dissolved in six ounces of water, were injected into the stomach of a dog by means of a tube of elastic gum, passed down the œsophagus. In three minutes he vomited a small quantity of mucus; and this occurred again several times. The pulse became

* I have observed, that where the functions of the brain are disturbed, paralysis first takes place in the muscles of the hind legs; afterwards in those of the trunk and fore legs; and last of all in the muscles of the ears and face. These facts seem to show, that the influence of the brain, like that of the heart, is not propagated with the same facility to the distant as to the near organs; and this is further confirmed by cases of disease which occasionally occur, in which, although the paralysis is confined to the lower half of the body, the morbid appearances met with on dissection are confined to the brain.

less frequent, and occasionally intermitted. At the end of 35 minutes, the hind legs were paralysed, and he lay in a half sensible state. At the end of 45 minutes he was less sensible, the pupils of the eyes were dilated, the pulse had fallen from 140 to 70 in a minute, and the intermissions were frequent. After this he became quite insensible, convulsions took place, and, at the end of 50 minutes from the beginning of the experiment, he died.

On opening the thorax immediately after death, tremulous contractions of the heart were observed; but not sufficient to maintain the circulation. The stomach and intestines contained a large quantity of mucous fluid, and their internal membrane was highly inflamed.

These experiments were repeated, and the results, in all essential circumstances, were the same. The symptoms produced were: 1. Paralysis of the hind legs, and afterwards of the other parts of the body; convulsions; dilatation of the pupils of the eyes; insensibility: all of which indicate disturbance of the functions of the brain. 2. A feeble, slow, intermitting pulse, indicating disturbance of the functions of the heart. Where the heart has continued to act after apparent death, I have never, in any one instance, been able to prolong its action by means of artificial respiration. 3. Pain in the region of the abdomen; preternatural secretion of mucus from the alimentary canal; sickness and vomiting in those animals which are capable of vomiting;

symptoms which arise from the action of the poison on the stomach and intestines. There is no difference in the effects of arsenic, whether it is employed in the form of oxyde, or of arseniac acid, except that the latter is a more active preparation. When arsenic is applied to a wound, the symptoms take place sooner than when it is given internally; but their nature is the same.

The symptoms produced by arsenic may be referred to the influence of the poison on the nervous system, the heart*, and the alimentary canal. As of these, the two former only are concerned in those functions which are directly necessary to life; and as the alimentary canal is often affected only in a slight degree, we must consider the affection of the heart and nervous system as being the immediate cause of death.

In every experiment which I have made with

* When I say that a poison acts on the heart, I do not mean to imply that it necessarily must act directly on the muscular fibres of that organ. It is highly probable, that the heart is affected only through the medium of its nerves; but the affection of the heart is so far independent of the affection of the nervous system generally, that the circulation may cease, although the functions of the brain are not suspended, and the functions of the brain may be wholly suspended without the circulation being at all disturbed. In proof of the first of these propositions, I may refer to my former experiments on the upas antiar, in which the sensibility of the animal continued to the very instant of death; and respiration, which is under the influence of the brain, continued even after the heart had ceased to act. In proof of the second, I may refer, among many others, to the experiments detailed in the Croonian Lecture for 1810.

arsenic there were evident marks of the influence of the poison on all the organs which have been mentioned; but they were not in all cases affected in the same relative degree. In the dog, the affection of the heart appeared to predominate over that of the brain; and on examining the thorax, immediately after death, this organ was found, to have ceased acting, and in a distended state. In the rabbit, the affection of the brain appeared to predominate over that of the heart, and the latter was usually found acting feebly, after the functions of the brain had entirely ceased. In the rabbit, the effects produced by the arsenic on the stomach and intestines were usually less marked than in carnivorous animals.

The action of arsenic on the system is less simple than that of the majority of vegetable poisons. As it acts on different organs, it occasions different orders of symptoms, and as the affection of one or another organ predominates, so there is some variety in the symptoms produced even in individual animals of the same species.

In animals killed by arsenic, the blood is usually found fluid in the heart and vessels after death; but otherwise all the morbid appearances met with on dissection are confined to the stomach and intestines. As this is the case, and as the affection of these organs is, in many respects, very remarkable, I shall mention the result of my observations on this subject.

In many cases where death takes place, there

is only a very slight degree of inflammation of the alimentary canal: in other cases the inflammation is considerable. It generally begins very soon after the poison is administered, and is greater or less according to the time which elapses before the animal dies. Under the same circumstances, it is, as I have already observed, less in rabbits than in carnivorous animals. The inflammation is greatest in the stomach and intestines; but it usually extends also over the whole intestine. I have never observed inflammation of the œsophagus. It *is greater in degree and more speedy in taking place, when arsenic is applied to a wound than when it is taken into the stomach.* The inflamed parts are in general universally red, at other times they are red only in spots. The principal vessels leading to the stomach and intestines are much dilated, and turgid with blood; but the inflammation is usually confined to the mucous membrane of these viscera, which assumes a florid red colour, becomes soft and pulpy, and is separable without much difficulty from the cellular coat, the latter presenting its natural appearance. In some instances, there are small spots of extravasated blood on the inner surface of the mucous membrane, or immediately beneath it, and this occurs independently of vomiting. I have never, in any of my experiments, found ulceration or sloughing of the stomach or intestine; but if the animal survives for a certain length of time, after the inflammation, has begun, it is reasonable to conclude

that it may terminate in one or other of these ways.

I am disposed to believe that sloughing is very seldom, if ever, the direct consequence of the application of arsenic to the stomach or intestines. Arsenic applied to an ulcer will occasion a slough; but its action in doing this is very slow. When I have applied the white oxyde of arsenic to a wound, though the animal has sometimes lived three or four hours afterwards, and though violent inflammation has taken place in the stomach and intestines, I have never seen any preternatural appearance in the part to which it was applied, except a slight effusion of serum into the cellular membrane. The very copious secretion of mucus and watery fluid which arsenic speedily produces from the stomach and intestines, separates it from actual contact with the inner surface of these organs, even though taken in large quantity and in substance; and, in animals which are capable of vomiting, by much the greater part is rejected from the stomach very soon after it has been taken in. Hence, though a few particles of arsenic are sometimes found entangled in the mucus, or in the coagulum of extravasated blood, and adhering to the inner surface of the stomach, I have never seen it in such a quantity as might be supposed capable of destroying the vitality of the parts with which it had been in contact. In one instance, where a dog had swallowed a large quantity of arsenic in substance, a brown spot,

about an inch in diameter, was observed after death on the inner surface of the cardiac extremity of the stomach, having so much of the appearance of a slough, that at first I had no doubt of it being so; but on examination, this proved to be only a thin layer of dark coloured coagulum of blood, adhering very firmly to the surface of the mucous membrane, and having a few particles of arsenic entangled in it. On removing this, the mucous membrane still appeared of a dark colour; but this also was found to arise from a thin layer of coagulum of blood between it and the cellular coat. The mucous membrane itself was inflamed; but otherwise in a natural state. I have observed a similar appearance, but occupying a less extent of surface, several times. In the Hunterian Museum there is a human stomach, which was preserved to show what was considered as a slough produced by the action of arsenic. On examining this preparation, I found that the dark coloured spot, which had been supposed to be a slough, was precisely of the same nature with that just described.

Although the affection of the stomach and intestines from arsenic is not the cause of death, under ordinary circumstances, it is reasonable to conclude, that it may be so in some instances, if the animal survives the effects produced on the organs more immediately necessary to life. Mr. Henry Earle informed me of an instance, in which this appeared to be the case. A woman

in St. Bartholomew's hospital, who had taken arsenic, recovered of the immediate symptoms, but died at the end of four or five days. On examination after death, extensive ulcerations were found of the mucous membrane of the stomach and intestines, which we can hardly doubt to have been the cause of death.

It is an important matter of inquiry, as connected with judicial medicine, how far may the examination of the body, after death, enable us to decide, whether an animal has died of the effects of arsenic? On this subject, however, I have only a few remarks to make.

The inflammation from arsenic, occupying in general the whole of the stomach and intestine, is more extensive than that from any other poison with which I am acquainted. It does not, as I have already stated, affect the pharynx or œsophagus, and this circumstance distinguishes it from the inflammation occasioned by the actual contact of irritating applications.

In my experiments I have not obtained much information from the examination of the contents of the stomach after death. When arsenic has been taken in substance, small particles of it are frequently found entangled in the mucus, or in the extravasated blood; but where this was not the case, I have never known, in an animal capable of vomiting, that arsenic could be detected in the stomach afterwards. As some substances, when taken internally, are separated from the blood very soon afterwards with the

urine, I thought it probable that arsenic might be separated with the urine also; but Mr. Brande (to whom I am indebted for assistance on the present, as I have been on many other occasions) could never detect the smallest trace of arsenic in it.*

IV. *Experiments with the Muriate of Barytes.*

When barytes is taken into the stomach, or applied to a wound, it is capable of destroying life; but when in its uncombined state its action is very slow. The muriate of barytes, which is much more soluble than the pure earth, is (probably on this account) a much more active poison.

EXPERIMENT V.

Ten grains of muriate of barytes rubbed very fine, and moistened with two drops of water, were applied to two wounds in the thigh and side of a rabbit. In four minutes he was evidently under the influence of the poison. In a short time he became giddy: then his hind legs were paralysed; and he gradually fell into a state of insensibility, with dilated pupils, and lay in general motionless, but with occasional convulsions. The pulse beat 150 in a minute, but feebly, and it occasionally intermitted. He

* See Additional Note H.

was apparently dead in 20 minutes from the time of the application of the poison; but on opening the chest, the heart was found still acting, and nearly three minutes elapsed before its action had entirely ceased.

EXPERIMENT VI.

An ounce and a half of saturated solution of muriate of barytes was injected into the stomach of a full grown cat, by means of an elastic gum tube. In a few minutes it operated as an emetic. The animal became giddy, afterwards insensible, and lay with dilated pupils, in general motionless, but with occasional convulsions. At the end of 65 minutes from the beginning of the experiment, he was apparently dead; but the heart was still felt through the ribs, acting 100 times in a minute. A tube was introduced into the trachea, and the lungs were inflated about 36 times in a minute; but the pulse sunk notwithstanding, and at the end of seven minutes the circulation had entirely ceased.

From these experiments, I was led to conclude, that the principal action of the muriate of barytes is on the brain; but, in the first the pulse was feeble and intermitting; in the second, although the artificial respiration was made with the greatest care, the circulation could not be maintained longer than a few minutes. These circumstances led me to suspect, that although this poison operates principally on the brain, it

operates in some degree on the heart also. Further experiments confirmed this suspicion. In some of them the pulse soon became so feeble, that it could be scarcely felt; and its intermissions were more frequent; but in all cases the heart continued to act after respiration had ceased; and the cessation of the functions of the brain was therefore always the immediate cause of death. When I employed artificial respiration, after death had apparently taken place, I seldom was able to prolong the heart's action beyond a few minutes. In one case only it was maintained for three quarters of an hour. I never by these means succeeded in restoring the animal to life, although the experiments were made with the greatest care and in a warm temperature. In some instances, after the artificial respiration had been kept up for some time, there were signs of the functions of the brain being in some degree restored; but the pulse, notwithstanding, continued to diminish in strength and frequency, and ultimately ceased. I shall detail one of these experiments, as it serves to illustrate the double action of this poison on the nervous and vascular systems.

EXPERIMENT VII.

Some muriate of barytes was applied to a wound in the side of a rabbit. The usual symptoms took place, and at the end of an hour the animal was apparently dead; but the heart still

continued to contract. He was placed in a temperature of 80°, and a tube being introduced into the nostril, the lungs were artificially inflated about 36 times in a minute.

When the artificial respiration had been maintained for four minutes, he appeared to be recovering; he breathed voluntarily 100 times in a minute, and showed signs of sensibility. The artificial respiration was discontinued. The voluntary respiration continued about nine minutes, when it had ceased, and the animal was again apparently dead; but the pulse continued strong and frequent. The lungs were again artificially inflated. At the end of four minutes the animal once more breathed voluntarily 100 times in a minute, and repeatedly moved his limbs and eye-lids. The pulse became slower and more feeble.

In a few minutes the voluntary respiration again ceased, and the artificial respiration was resumed. The pulse had fallen to 100, and was feeble. The animal again breathed voluntarily; but he ceased to do so at the end of five minutes. The lungs were inflated as before; but he did not give any sign of life, nor was the pulse felt afterwards. On opening the thorax, the heart was found to have entirely ceased acting.

A probe having been introduced into the substance of the spinal chord, it was found that by means of the Voltaic battery, powerful contractions might be excited, not only of the voluntary

muscles, but also of the heart and intestines; from which it may be inferred, that the muriate of barytes, like arsenic, affects the circulation by rendering the heart insensible to the usual stimulus, and not by destroying altogether the power of muscular contraction.

The muriate of barytes affects the stomach, but in a less degree than arsenic. It operates as an emetic in animals that are capable of vomiting; but sooner when taken internally, than when applied to a wound. In general, but not constantly, there are marks of inflammation of the inner membrane of the stomach, but not of the intestine. In many instances there is a thin layer of dark coloured coagulum of blood lining the whole inner surface of the stomach, and adhering very closely to it, so as to have a good deal of the appearance of a slough; and this is independent of vomiting, as where I met with it, it occurred in rabbits.

The same circumstances, from which it may be inferred that arsenic does not produce its deleterious effects until it has passed into the same circulation, lead also to that conclusion with regard to the muriate of barytes.

V. *On the Effects of the Emetic Tartar.*

The effects of the emetic tartar, so much resemble those of arsenic and of muriate of barytes in essential circumstances, that it would be need-

less to enter into a detail of the individual experiments made with this poison.

When applied to a wound in animals, which are capable of vomiting, it usually, but not constantly, operates very speedily as an emetic; otherwise, I have found no material difference in the symptoms produced in the different species of animals, which I have been in the habit of employing as the subjects of experiment. The symptoms are paralysis, drowsiness, and at last complete insensibility; the pulse becomes feeble; the heart continues to act after apparent death; its action may be maintained by means of artificial respiration, but never for a longer period than a few minutes: so that it appears that this poison acts on the heart as well as on the brain; but that its principal action is on the latter. Both the voluntary and involuntary muscles may be made to contract after death, by means of Voltaic electricity. The stomach sometimes bears the marks of inflammation; but at other times it has its natural appearance. I have never seen any appearance of inflammation of the intestines. The length of time which elapses from the application of the poison to the death of the animal, varies. In some instances, it is not more than three quarters of an hour; but in others, it is two or three hours, or even longer.

When a solution of emetic tartar was injected into the stomach of a rabbit, the same symptoms took place as when it was applied to a wound.

VI. *On the Effects of the Corrosive Sublimate.*

When this poison is taken internally in very small and repeated doses, it is absorbed into the circulation, and produces on the system those peculiar effects which are produced by other preparations of mercury. If it passes into the circulation in larger quantity, it excites inflammation of some part of the alimentary canal, the termination of which may vary accordingly as it exists in a greater or less degree. When taken in a larger quantity still, it occasions death in a very short space of time. I had found, that if applied to a wounded surface, it produced a slough of the part to which it was applied, without occasioning any affection of the general system. This led me to conclude that the effects of it, taken internally, and in a *large* quantity, depend on its local action on the stomach, and that they are not connected with the absorption of it into the circulation. The following experiments appear to confirm this opinion.

EXPERIMENT VIII.

Six grains of corrosive sublimate, dissolved in six drachms of distilled water, were injected into the stomach of a rabbit, by means of an elastic gum tube. No immediate symptoms followed the injection; the animal made no expression of pain; but in three minutes he became insensible;

was convulsed; and in four minutes and a half, from the time of the injection being made, he died. Tremulous contractions of the voluntary muscles continued for some time afterwards. On opening the thorax, the heart was found to have entirely ceased acting, and the blood in the cavities of the left side was of a scarlet colour. The stomach was much distended. The pyloric and cardiac portions were separated from each other by a strong muscular contraction. The contents of the former were firm and solid, and in every respect resembled the usual contents of the stomach; while those of the cardiac portion consisted of the food of the animal much diluted by fluid; so that the solution, which had been injected, appeared to be confined to the cardiac portion of the stomach, and to be prevented entering the pyloric portion by the muscular contraction in the centre.

In the pyloric portion of the stomach the mucous membrane had its natural appearance; but in the cardiac portion it was of a dark grey colour, was readily torn, and peeled off; and in some parts its texture was completely destroyed, so that it appeared like a pulp, on removing which the muscular and peritonæal coats were exposed.

The repetition of the experiment was attended with similar results. The alteration of the texture of the internal membrane was evidently occasioned by its having been chemically acted on by the corrosive sublimate injected into it.

When the injection is made into the stomach of a dead rabbit, precisely the same effects are produced, except that, as the middle contraction is here wanting, the appearances are not confined in the same degree to the cardiac portion.

EXPERIMENT IX.

A scruple of corrosive sublimate, dissolved in six drachms of distilled water, was injected into the stomach of a full-grown cat. For the first five minutes no symptoms were produced. After this, the poison operated twice as an emetic. The animal was restless and excited, then gradually became insensible, and lay on one side motionless, with the pupils of the eyes dilated. The respiration was laborious, and the pulse could not be felt. Twenty-five minutes after the poison had been injected, there was a convulsive action of the voluntary muscles, and death immediately ensued. On opening the thorax immediately afterwards, the heart was seen still contracting, but very feebly.

The stomach was found perfectly empty and contracted. The mucous membrane was every where of a dark grey colour. It had lost its natural texture, and was readily torn and separated from the muscular coat. The internal membrane of the duodenum had a similar appearance, but in a less degree, for nearly three inches from the pylorus. In the situation of

the pylorus, the effects of the poison were less apparent than elsewhere.

The peculiar state of the internal membrane of the stomach, in this experiment as well as in the last, appears to have been occasioned by the chemical action of the poison on it. When I injected a solution of corrosive sublimate into the stomach of a dead cat, and retained it there for a few minutes, a similar alteration of the texture of the internal membrane took place; but it assumed a lighter grey colour. The difference of colour may be explained by the vessels in the one case having been empty, and in the other case having been distended with blood at the time of the injection being made.

The destruction of the substance of the internal membrane of the stomach precludes the idea of the poison having been absorbed into the circulation. We must conclude that death was the consequence of the chemical action of the poison on the stomach. This organ, however, is not directly necessary to life, since its functions, under certain circumstances, are suspended for hours, or even for days, without death being produced. Although the stomach was the part primarily affected, the immediate cause of death must be looked for in the cessation of the functions of one or more of those organs whose constant action is necessary to life. From the scarlet colour of the blood in the left side of the heart, in the experiment on the rabbit, we may conclude that the functions of the lungs were

not affected; but the affection of the heart and brain is proved by the convulsions, the insensibility, the affection of the pulse in both experiments, and the sudden cessation of the heart's action in the first, and we may therefore be justified in concluding, that the immediate cause of death was in both of these organs. As the effects produced appear to have been independent of absorption, we may presume that the heart, as well as the brain, was acted on through the medium of the nerves.

That a sudden and violent injury of the stomach should be capable of thus speedily proving fatal is not surprising, when we consider the powerful sympathy between it and the organs on which life more immediately depends, and the existence of which many circumstances in disease daily demonstrate to us.

VII. *Conclusion.*

The facts which have been stated appear to lead to the following conclusions respecting the action of the mineral poisons, which were employed in the foregoing experiments.

1. Arsenic, the emetic tartar, and the muriate of barytes do not produce their deleterious effects until they have passed into the circulation.

2. All of these poisons occasion disorder of the functions of the heart, brain, and alimentary

canal; but they do not all affect these organs in the same relative degree.

3. Arsenic operates on the alimentary canal in a greater degree than either the emetic tartar, or the muriate of barytes. The heart is affected more by arsenic than by the emetic tartar, and more by this last than by the muriate of barytes.

4. The corrosive sublimate, when taken internally in large quantity, occasions death by acting chemically on the mucous membrane of the stomach, so as to destroy its texture; the organs more immediately necessary to life being affected in consequence of their sympathy with the stomach.

In making the comparison between them, we may observe that the effects of mineral are less simple than those of the generality of vegetable poisons; and it appears that when once an animal is affected by the former, there is much less chance of his recovery than when he is affected by the latter.

ADDITIONAL NOTES.

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Note A. (Page 36.)

In the experiments related in my two memoirs on the production of animal heat, it has been seen that the animals in which the circulation of the blood was maintained by means of artificial respiration after the removal of the brain, or the destruction of its functions by the agency of a narcotic poison, cooled more rapidly than those in which the circulation had entirely ceased.

The general result as to the loss of heat in animals under these circumstances has been confirmed by the observations of other experimentalists, especially of Dr. Chossat, M. Le Gallois, and Dr. Wilson Philip. It is, however, stated by M. Le Gallois, that in his experiments the loss of heat was not uniform, and that while in some instances it was greater in the animal under the influence of artificial respiration than in the dead animal, in other instances it was the reverse, the latter having preserved a somewhat higher temperature than the former.

I know that my observations were made with great care, at the same time that they were con-

firmed by those who assisted me on these occasions; and I do not doubt that those of M. Le Gallois were equally correct. Dr. Wilson Philip has pointed out what is probably one cause of the difference between us, in the difference in the quantities of air impelled into the lungs. In a rabbit in which the lungs were inflated about 30 times in a minute (this being about the ordinary number of respirations in a full-grown animal of this species), the loss of heat was greater than in the dead rabbit; but when the process was repeated only 12 times in a minute, the proportion was reversed, and the dead animal cooled somewhat faster than the other.

One effect of the admission of cold air into the lungs, whether by the natural process of respiration, or by respiration artificially performed, must be to carry off from the animal a certain portion of heat; and this effect must be produced to a greater extent if, when respiration is artificially performed, a larger quantity of cold air enters the lungs than that which they receive under ordinary circumstances. It is difficult to estimate the precise quantity of air which enters the lungs when they are artificially inflated; and I must therefore admit that one of the conclusions which I ventured to draw from my experiments, — namely, that where the influence of the brain is withdrawn, no heat whatever is generated, — may not be strictly and rigidly correct. The difference, however, in this respect between these two modes of respiration,

where the experiments are conducted with any degree of care, is certainly but trifling, and not such as will account for the great diminution of temperature which takes place. In confirmation of what is now stated, I may refer to the following experiment made by Dr. Chossat.* Having perforated the cranium of a dog, by means of a small trephine, he made a section of the brain immediately in front of the *Pons Varolii*, so as to intercept the communication of the nervous influence from the rest of the brain to the last mentioned organ. The animal continued to breathe by his natural efforts, the pulse and respiration being for many hours as frequent as under ordinary circumstances. Nevertheless at the end of four hours he had lost 9·7° (centigrade) of heat; while, in another experiment, a dog, a very little larger, which had been killed by the division of the spinal chord in the upper part of the neck, lost 8·8° (centigrade) in the same period of time. It may be further observed, that my experiments were all made during the warm weather of summer, when the cooling property of the air must have been much less than in the cold season of the year, and that, whatever might have been the influence in this respect of the artificial respiration in my first series of experiments, it must have been very trifling in my second series, not only because, these last experi-

* Mémoire sur l'Influence du Système Nerveux sur la Chaleur Animale, p. 15. 1820.

ments having been made in a close vessel, the cold air inspired must necessarily have been always mixed with a certain portion of the air expired previously, but also because, from the relative position of the animal and the aperture in the upright portion of the tube, the air drawn into the latter by the elasticity of the gum-bottle, and afterwards impelled into the lungs, must have had communicated to it a large portion of the heat which the entire body of the animal was gradually losing.

According to the then received theory of the production of animal heat, the loss of heat in these experiments cannot be accounted for, except by assuming that there was a very much smaller quantity of oxygen consumed, and of carbonic acid generated, than where respiration is performed under ordinary circumstances. Such an assumption, however, even as to my first series of experiments, is not supported by the facts. The dark venous blood, after it had passed through the lungs, underwent the usual change of colour; and not only the contractions of the heart were regularly performed, but, in dogs especially, there seemed to be actually an increased irritability of the voluntary muscles, continued, not for a short time, but even for an hour and a half, and until the temperature of the rectum had fallen to 15° (Fahrenheit) below the natural standard. These muscular contractions were most remarkable, in one of the experiments related in my first paper on poisons, after the

injection of the infusion of tobacco into the intestine, keeping not only the extremities but the whole body of the animal in a state of constant motion, and lasting for some minutes after the artificial respiration had been discontinued and the blood had ceased to circulate.

It was with a view to obtain more exact information as to the quantity of oxygen consumed under these circumstances, that my second series of experiments was instituted. The apparatus employed in the investigation proved to be well adapted for the intended purpose; the experiments were carefully made; and the air, after they were concluded, having been always examined by my friend Professor Brande, there can be no doubt as to the accuracy with which the proportion of carbonic acid in it was ascertained.

These last-mentioned experiments were repeated, in the year following that in which an account of them was published in the Philosophical Transactions, by M. Le Gallois; with some considerable modifications, however, which I shall notice hereafter. In his third memoir on animal heat, this philosopher, after having described his own want of success in contriving an apparatus for the purpose of measuring the quantity of oxygen consumed by animals breathing by artificial means, observes: "M. Brodie a trouvé le moyen de pratiquer l'insufflation pulmonaire dans des vaisseaux fermés à une manière à la fois sure et commode. L'appareil qu'il a imaginé pour cela est fort simple, et réunit toutes les

conditions, qu'on pourrait désirer pour le succès de l'expérience."

In using this apparatus, it has been seen that I employed rabbits of nearly the same size, occupying the space of from 48 to 50 cubic inches. My three first experiments were intended to determine the quantity of carbonic acid exhaled from the lungs of rabbits of this size, breathing under natural circumstances. In one of them it proved to be 25·3 cubic inches, in each of the others 28·22 cubic inches in half an hour, giving an average of about 27·25 cubic inches of carbonic acid in this space of time. In my third experiment, a rabbit, in which the spinal chord, nerves, and vessels had been so divided as to produce the same effect as decapitation, the circulation having been afterwards maintained by artificial respiration, generated 20·2 cubic inches in half an hour. The formation of carbonic acid was, therefore, less than that by the animal in its natural condition, in the proportion of about 20 to 27. But in this experiment an amount of hæmorrhage took place, which, although immediately suppressed, seemed to afford a sufficient explanation of the difference. In order to avoid this source of error, my subsequent experiments were made in a way in which no loss of blood could ensue, the functions of the brain being destroyed, not by any mechanical operation, but by means of a narcotic poison. The result was, that the carbonic acid generated was fully equal to that which rabbits of the same size supply under natural circumstances.

In the experiments which M. Le Gallois instituted with my apparatus, he employed very young rabbits, much smaller than those which had been employed by myself. Instead of placing them under the influence of a narcotic poison, or of decapitation, in some of them he merely made a section of the spinal chord, immediately below the cranium; while in others, besides dividing the chord, he destroyed the texture of the brain, by introducing an instrument into the cranium, through the great foramen of the occiput. After having inflated the lungs for 30 minutes, he found that the carbonic acid generated was, in the majority of instances, considerably less than that which was generated by the same animals breathing naturally; and comparing these experiments with those which I had made with the narcotised animals, he states that the results of his experiments do not correspond with those obtained by myself. I cannot, however, admit the justice of this comparison. I attributed the smaller production of carbonic acid in the first of my experiments to the loss of blood consequent on the division of the spinal chord and of the vessels and nerves in the neck. We know that the section of the spinal chord below the occiput is in itself sufficient to cause immediately a certain degree of hæmorrhage from the muscles of the neck, and from the arteries, veins, and sinuses within the theca vertebralis. But where an instrument is introduced through the foramen of the occiput, for the purpose of destroy-

ing the texture of the brain, there cannot fail to be a still larger hæmorrhage, from the laceration of the arterial circle formed by the carotid and vertebral arteries in the base of the brain. In either case the hæmorrhage would probably continue for some time after the artificial respiration was begun, and must considerably influence the result of the experiment. As I have already stated, it was to avoid a similar source of error that I had recourse to the method of destroying the functions of the brain by means of narcotic poison, and it is plain that in comparing the results of our respective experiments, it is not with these last, but only with the first of my experiments, that the comparison should be made.

The experiments of M. Le Gallois having been made on much younger and smaller animals than those employed by myself, the question is not as to the absolute quantity of air consumed where the influence of the brain is destroyed, but as to the proportion which it bears to the air consumed by animals of the same kind and size breathing under ordinary circumstances. Of his experiments, five in number, the results were very different. In one instance, a rabbit which, breathing under natural circumstances, had generated 142·43 cubic centimetres in 30 minutes, when under the influence of artificial respiration generated as much as 137·8 cubic centimetres in the same space of time; while in another the corresponding numbers were 259·8,

and 206·5; and 209·16 and 109·99 in a third. This difference is probably to be attributed, in great measure, to the greater or less extent of the internal hæmorrhage. If we take the average of the whole of M. Le Gallois's experiments, we find that the carbonic acid produced by natural respiration, in 30 minutes, was 233·94, and that by artificial respiration 181·67 cubic centimetres; the proportion of the latter to the former being actually larger than it was in the corresponding experiment made by myself.* Taking all these circumstances into consideration, I apprehend that the results obtained by M. Le Gallois, instead of contradicting, must be considered as confirming those which I had obtained previously, and as justifying the opinion that if he had conducted his experiments so as to avoid the disturbing influences connected with the mechanical destruction of the functions of the brain, he would have found, as I did, that the exhalation of carbonic acid, when respiration was performed artificially, fully equalled that which took place when the animal was in its natural condition.

In pursuing these investigations, I assumed that the carbonic acid, generated in respiration, may be taken as the measure of the oxygen con-

* If we suppose 100 to represent the quantity of air consumed in natural respiration, the proportion borne by that to the air consumed in artificial respiration in my first experiment after the division of the spinal chord, nerves, and vessels in the neck, was nearly as 100 : 74; and in M. Le Gallois' experiments, as 100 : 77.

sumed, this being the conclusion to which Messrs. Allen and Pepys were led by their carefully conducted investigations. It has, however, been stated by others, that a portion of oxygen disappears beyond that which is replaced by the carbonic acid, and M. Le Gallois endeavoured to ascertain to what extent this loss of oxygen takes place. The quantity, according to his statement, was so much greater than that which has been found by others, that it is very difficult to avoid the suspicion that there was some error in the observations. However, admitting them to be correct, as (the experiments having been made in the same way, and with the same apparatus), whatever oxygen was absorbed in his experiments, a corresponding quantity must have been absorbed in mine, and as, according to M. Le Gallois's statements, the proportion of oxygen absorbed by animals respiring naturally, and by those respiring artificially, though differing in individual instances, did not, on an average, materially differ from that of the carbonic acid generated, the remarks which I have made as to the latter are equally applicable to the former, so that we are led to no different conclusion.

It has been seen that, in my last series of experiments, the animals under the influence of artificial respiration cooled somewhat more

rapidly than the dead animals, the difference between them, however, being less than in my former experiments, probably in consequence of their having been carried on in a close vessel, and not in the open air.

It has also been seen that in some of the experiments made by M. Le Gallois with decapitated animals, and described in his first memoir, the animal respiring artificially cooled more rapidly, and in others more slowly, than the dead animal, while in others there was no appreciable difference in this respect between them. But in those described in his last memoir, and made with my apparatus, the dead animal always cooled more slowly than the other, the difference varying from as little as 0·3° centigrade to as much as 2·3° centigrade (that is, from a little more than half a degree to about 4 degrees of Fahrenheit), in half an hour.

It certainly may be that the discrepancy in this respect between these last-mentioned experiments on the one hand, and my own and some of M. Le Gallois's earlier experiments on the other hand, may have arisen from the cause pointed out by Dr. Philip, namely, the greater or less degree in which the lungs were inflated. It seems to me, however, that it admits of another and a more probable explanation.

In fact, M. Le Gallois's last experiments were not an exact repetition either of my experiments, or of his own former experiments. In some of them the spinal chord was divided, and the brain

was left entire. In others the brain was mechanically disorganised by means of an instrument introduced through the foramen of the occiput. In the former case it is evident that the brain must have retained its influence over the head, and to a certain extent over the rest of the system, by means of the pneumogastric and great sympathetic nerves; and it is by no means certain that the disorganisation of the brain must have been always so complete as wholly to destroy its influence in the latter.* Further experiments, indeed, are required before this question can be positively determined. The main fact, however, remains the same, namely, that under certain circumstances, there is no relation between the

* Some experiments made by Dr. Enoch Hale (of the United States), are recorded in the London Medical and Physical Journal for 1815, in which, having merely divided the spinal chord immediately below the occiput, and having maintained the action of the heart by artificial respiration, he found that a small degree of heat was still generated. As the brain and nerves in the neck remained entire, it is plain that these experiments are liable to the objection which has been stated above. It may, moreover, be observed that in such experiments it ought to be ascertained afterwards whether the division of the spinal chord has been complete, as the result of accidental injuries shows that even a partial laceration of the spinal chord may produce total paralysis of the parts below the seat of the injury. In another respect also Dr. Hale's experiments cannot be regarded as a repetition of my own, having been made on dogs; whereas my comparative experiments were all made on rabbits. M. Le Gallois found that young cats produced a small degree of heat after decapitation; and it may possibly be the same with respect to dogs.

oxygen consumed in respiration, and the heat generated, while there is a very close relation between the calorific function and the integrity of the functions of the nervous system. This is, indeed, admitted by M. Le Gallois himself, when he thus expresses himself: "Il n'y a pas de doute que la puissance nerveuse ne joue un très grand rôle dans ce phénomène, de même que dans tous ceux qui supposent la vie."* And again, "C'est sur la conversion du sang artériel en sang veineuse, et sur le changement de capacité qui l'accompagne, que la puissance nerveuse a une action immédiate. Aussi remarque-t-on que le développement du calorique, soit dans tout le corps, soit dans une partie déterminée, est en raison de l'énergie de cette puissance marquée par l'activité de la circulation."†

At the time when these researches were instituted the theory enunciated by Lavoisier and La Place was universally received, and no one entertained a doubt that the temperature which a warm-blooded animal possesses above that of the atmosphere was entirely explained by the consumption of oxygen in respiration. Since then further inquiries have been made on the subject by several distinguished chemists; and the observations of M. Dulong, M. Despretz, and very lately those of Messrs. Regnault and Reiset, have confirmed the opinion that this expla-

* Œuvres de Le Gallois, tome ii. p. 53.
Ibid. p. 55.

nation is insufficient, and that whatever the effect of the conversion of oxygen into carbonic acid may be, there must be some other source of animal heat.

I shall conclude this note with adducing some other facts, which lead to the same conclusion, and which indeed seem to be in themselves sufficient to prove that the production of animal heat is a subject which requires still further investigation.

1. The various animals of the class Mammalia have nearly the same temperature, the difference between them being not more than one or two degrees of Fahrenheit's thermometer. There is, however, reason to believe that there is a great difference as to the quantity of oxygen which they consume in respiration. Thus M. Le Gallois found, that in the space of 3 hours a rabbit, weighing 947 grammes, consumed only 2724 cubic centimetres; while a dog, weighing 917 grammes, consumed 5503 cubic centimetres; and a cat, weighing 634 grammes, consumed as much as 3963 cubic centimetres in the same space of time.*

2. In the course of his researches, M. Le Gallois found that living rabbits, merely from being confined in a restrained position on the back, gradually became of a lower temperature; and he observes, that if this experiment were continued for a considerable length of time, the cooling

* Œuvres de Le Gallois, tome ii.

process might be carried so far as even to occasion the death of the animal. Supposing that the loss of heat under these circumstances must be connected with a diminished consumption of oxygen in respiration, he repeated the experiments, having placed the animals in an apparatus in which the respired air might be preserved and examined. The result, however, did not correspond with his anticipations; the amount of oxygen consumed being found to vary in different instances, so that it was sometimes much less, and at other times much greater, than under ordinary circumstances. M. Le Gallois attributes the loss of heat in these experiments, partly to the greater effort required for breathing in the position in which the animals were placed, and partly to the exertions of the muscular system generally made to obtain relief from the restraint. Neither of these, however, can be regarded as a satisfactory explanation. The respiration of a man, or a horse, in rapidly ascending a hill, is carried on with much greater effort than when moving on a plain surface; but the result is not a diminution but an increase of temperature; and the very curious researches of Messrs. Becquerel and Breschet have demonstrated that there is actually a development of heat during muscular contraction. The more probable explanation seems to be that the loss of heat is the consequence of the state of alarm in which the animal is placed, operating on him in the same manner

as the same depressing passion operates on the human subject.

3. In the experiments of Messrs. Allen and Pepys*, it was found that when the air used in respiration was pure oxygen, a very much larger proportion of carbonic acid was generated than in the respiration of atmospheric air. A guinea pig, which furnished 0·88 cubic inch of carbonic acid per minute when breathing atmospheric air, furnished 1·48 cubic inch per minute in one experiment, and 1·11 cubic inch in another, when breathing pure oxygen. A corresponding difference, but to a less extent, was observed as to the results of respiration with atmospheric air and that with oxygen gas in the human subject. It does not appear that these experimentalists observed that the increase in the quantity of carbonic acid was attended with any increase of temperature; and in some very curious experiments, which were made (by my suggestion) by the late Mr. S. D. Broughton, and which are recorded in the Quarterly Journal of Science, it was found that the temperature of animals, which breathed pure oxygen, not only did not rise above the natural standard, but actually fell considerably below it.

4. While certain læsions of the nervous system cause a diminution of the vital temperature, the following history justifies the conclusion that there are others which may produce the opposite

* Phil. Trans. 1808, pp. 415. 417. 420.

effect, and cause its elevation above the natural standard. A man met with an accident which occasioned a forcible separation of the bodies of the fifth and sixth vertebræ of the neck, attended with an effusion of blood within the *theca vertebralis*, and a laceration of the lower part of the cervical portion of the spinal chord. It is well known to surgeons, that under such circumstances inspiration is always very imperfectly performed by means of the diaphragm only, without the assistance of the ribs; while, in consequence of the paralysed state of the abdominal muscles, expiration depends altogether on the pressure of the abdominal viscera acting on the relaxed diaphragm. The patient in this instance died at the end of 22 hours from the period of the injury, and for a considerable time previously to his death breathed at very long intervals, the pulse being weak, and the countenance livid. At last there were not more than 5 or 6 inspirations in a minute. Nevertheless, when the ball of a small thermometer was placed on the inside of the groin, the quicksilver rose to 111° of Fahrenheit. Immediately after death the temperature was examined in the same situation, and found to be still the same. The case occurred under my care in St. George's Hospital, in the year 1821, at which time Mr. Cæsar Hawkins was house surgeon; and the facts were observed and noted by Mr. Hawkins and myself, and witnessed by several of the students.

Although the elevation of temperature to so great an extent may not be a very frequent occurrence, cases must be in the recollection of every physician and surgeon, in which, under certain morbid conditions of the nervous system, the natural temperature was fully maintained, at the same time that the respiration was so slow and imperfect as to indicate that the consumption of oxygen in that process must have been greatly diminished. The following observations, illustrative of this subject, are taken from John Hunter's Lectures on the Principles of Surgery, delivered in the year 1786–1787*, and they are of so much interest, in connection with the present inquiry, that I am tempted to extract the whole passage.

"It being discovered that the absolute heat of bodies differs very much in different substances, and perhaps in the same substances differently combined, it was thought that this would account for the production, and continuation, of animal heat. It is supposed that the air which we inspire has much more absolute heat than the same air has when we expire it, and that the superabundant heat is given to the animal. But this chemical method of accounting for heat will not account for all the varieties in the heat of animals at different times, especially in disease, when the breathing does not equally vary, or correspond with the heat of the animal.

* Palmer's edition of the works of John Hunter, vol. i. pp. 282, 283.

"A remarkable case fell under my observation of a gentleman who was seized with an apoplectic fit. He lay insensible in bed, covered with the blankets. I found his whole body become extremely cold in every part, and continue so for some time, and in a short time become extremely hot. While this was going on for several hours alternately, there was no sensible alteration either in the pulse or in the breathing.

"A man fell from his horse, and pitched on his head, which produced all the symptoms of a violent injury. There was concussion of the brain, and perhaps extravasation of blood, but no fracture could be seen. The pulse was at first 120, but came down to 100, and was strong, full, and rather hard. He was very hot in the skin, but breathed remarkably slow, only one half of the common frequency. His breathing, pulse, and heat, therefore, did not correspond with this theory of heat.

"February, 1781. A boy, about three years old, appeared not quite so well as common, being attacked with a kind of shortness of breathing in the night. It had become exceedingly oppressed about five o'clock on Sunday morning, so difficult that he appeared dying for want of breath. In this state he lay till two o'clock, when I saw him. He was breathing so slowly, that I thought that every breath would be his last — about $2\frac{1}{2}$ or less inspirations in a minute. (The common rate of breathing in such a boy is about 30 inspirations in a minute, and about

20 in a man.) When he drew his breath, it was with a jerk; but his expiration was very slow, generally continuing 5 seconds. I often could not distinguish the pulse; at other times it was manifest, although very faint and slow, not more than 60 in a minute. On tying up the arm the vein did not rise in the least, so that the blood did not go its round. His eyes were turned up under the upper eyelid, while his body had a purple cast, especially the lips, which is easily accounted for, it being owing to the blood not getting the scarlet red in its passage through the lungs, and that whatever it might get there was lost in its slow motion through the arteries of the body. He had a fine warmth on the skin all over the body, although in a room without a fire, not covered with more clothes than common in the month of February, with snow falling at noon."

"Here it cannot be said that the heat of the body, which was neither great nor deficient, could arise from the constant supply furnished by respiration."

But here two questions will present themselves. If it be not from respiration, from what source is the higher temperature of warm-blooded animals derived? and where respiration is carried on without maintaining the vital temperature, what becomes of the heat, which under other circumstances is the result of the union of

carbon with oxygen gas, and their conversion into carbonic acid? Neither of these problems do I undertake to solve; my object being, as I have observed elsewhere, not to advance opinions, but simply to state some facts which I met with in the course of my physiological investigations. I venture, however, to offer the following suggestions: —

It has been shown by Mr. Grove* that there is great reason to believe that the physical forces to which we refer the ever varying phenomena of inorganic nature, such as light, heat, motion, magnetism, and chemical affinity, are in a state of mutual relation; so that under certain circumstances one of these forces is capable of producing, or being converted into, another. And Mr. Grove has further suggested that the principles and mode of reasoning on which his "observations are founded, may be applied to the organic as well as the inorganic world, and that muscular force, animal and vegetable heat, &c., may, and sometimes will be shown to have, similar definite correlations." Supposing these views to be correct, may it not be that the union of carbon with oxygen gas, which, under ordinary circumstances, is immediately followed by the evolution of heat, is, in the living body, productive of a different result, (such as the mainte-

* The Correlation of Physical Forces, by W. R. Grove, M. A., F. R. S., 2nd edition, 1850. See also Dr. Carpenter's Memoir "On the mutual Relations of the Vital and Physical Forces," in the Philosophical Transactions, 1850.

nance of the nervous power, or the irritability of the muscles,) and that it may thus be only indirectly concerned in the calorific function?

But setting these speculations aside, it may be observed that respiration is only one of many chemical processes which take place in the animal body, some of which may be supposed to cause an evolution of heat, and others to have a directly opposite effect; and if we refer to these, as connected with the higher temperature of warm-blooded animals, we should consider, not what may be the actual quantity produced by any one of them, but what is the amount produced on the whole, after making allowance for that which is wasted by perspiration, and in other ways.

If we cannot, on purely chemical principles, explain why the integrity of the nervous system is necessary to the maintenance of animal heat, neither can we explain the result of those very remarkable experiments lately made by M. Bernard, in which he found that a slight mechanical injury of a certain part of the *medulla oblongata*, causes a secretion of sugar by the kidneys; nor why one state of mind augments the secretion of the lachrymal gland, while another deranges the secretions of the stomach and liver.

But may not the fact admit of another explanation? However unsuccessful the attempts actually to identify the nervous power with electricity may have proved to be, there can be no

doubt that in many respects these two forces bear a considerable resemblance to each other. The generation of the nervous power as a consequence of the circulation of arterial blood in the grey matter of the brain and spinal chord; the instantaneous transmission of it through the nerves; sometimes affecting the secretions, and causing the blood to undergo changes in its chemical composition in the various glands, at other times exciting violent contractions of the muscles, — these things are very analogous to the effects produced by a voltaic battery; and there seems to be no reason, *à priori*, why the resemblance should not extend still further, nor why the evolution of heat should not be one of the results of the operation of the nervous power as it is of electricity.

Note B. (Page 43.)

In my papers on the mode in which death is produced by poisons, I have stated various circumstances which led me to the conclusion that poisons which are applied to wounded surfaces produce their effect by passing into the circulation; and that certain mineral poisons when introduced into the alimentary canal, have a similar mode of operation. There is no doubt

that various deleterious (as well as medicinal) agents of vegetable origin, when administered internally, also affect the vital organs in the same manner, by entering the vascular system, and being mixed with the circulating blood.

But I have further stated, that there is much reason to believe that some of the more active poisons operate in a different manner, and that when applied to mucous membranes they are capable of affecting the nervous centres through the medium of the nerves, and independently of absorption.

On this last point, other physiologists have arrived at a different conclusion, and are of opinion that even those poisons which are the most active, and which seem to produce their effect almost instantaneously, do so, not by an influence transmitted through the nerves, but solely by penetrating the solid tissues, and thus contaminating the blood.

The arguments in favour of this view of the subject have been well stated by Mr. Blake, in a memoir published in the 53rd volume of the Edinburgh Medical and Surgical Journal. They are founded on the facility with which the tissues are penetrated by fluid and gaseous bodies, and on the great rapidity of the circulation.

The facts advanced by Mr. Blake are equally interesting and important. There can be no doubt that poisons applied to the tongue may enter into the blood in a very short space of time; and that this must at any rate be one mode

in which they act; and that most of the phenomena which occur may be in this manner sufficiently explained.

Nevertheless, other facts may be adduced, which render it doubtful whether the whole of the phenomena admit of this explanation, at the same time that the analogy of what happens under other circumstances justifies us in regarding the agency of the nerves in transmitting the influence of certain poisons to the vital organs as no improbable hypothesis.

1. The rapidity with which the poison operated in some of my own experiments, in those of Dr. Christison made with the active principle of Hemlock, and of M. Magendie and Mr. Taylor with the hydrocyanic acid, is even greater than can be well accounted for otherwise, however rapid the circulation, and however easy the transmission of the poison may be through the substance of the mucous membranes and the tunicks of the blood-vessels.

2. In the first of my experiments on alcohol the introduction of two drams of proof spirits into the stomach of a cat immediately affected the nervous system to such an extent as to cause total insensibility with laboured and stertorous respiration; nevertheless after the lapse of eight minutes these symptoms began to subside, so that presently the animal was able to stand and walk. In another experiment, in which as much as an ounce and a half of proof spirit had been injected into the stomach of a full-grown rabbit,

the insensibility which it occasioned began to subside at the end of 40 minutes. It is easy to understand that the effects of the impression made by the poison on the sentient extremities of the nerves, like those of a concussion of the brain, should thus subside, but it does not seem very probable that so large a quantity of spirit should have been absorbed into the circulation and then ejected from it in so short a space of time; nor does this at all correspond with what happens where intoxication is gradually induced in the human subject, and where there can be no doubt as to the alcohol having entered the circulation.

3. That poisons may have a local action on the nerves, so as to affect distant organs, independently of their admission into the blood, is proved by the well known fact to which I have adverted elsewhere, of the pupil of one eye becoming dilated in consequence of the application of the extract of belladonna to the conjunctiva of the same eye, or the neighbouring part of the integuments, while the pupil of the other is wholly unaffected by it.

4. There are numerous examples of mechanical impressions on the sentient extremities of the nerves, the influence of which can be propagated only through the medium of the nerves themselves, affecting the brain so as to occasion a temporary suspension of its functions. Every practical surgeon will recal to his mind numerous instances of a common, simple, and bloodless

operation being followed by syncope, and even by stupor, with dilatation of the pupils, and stertorous respiration, and continuing for several minutes; and there is, *à priori*, no evident reason why the impression made on the nerves by so powerful an agent as the hydrocyanic acid, or the essential oil of bitter almonds, should not do in one case what a slight mechanical injury does in another.

5. In another publication I have referred to another class of cases, the phenomena of which cannot be well explained except by attributing them to an influence transmitted through the medium of the nerves. Thus, in one instance, acid in the stomach caused a severe pain in the foot, which was immediately relieved by a dose of alkali neutralizing the acid. In another instance a violent pain in the ankle, with inability to move, subsided immediately on the rejection of some indigestible food from the stomach. *

6. I have elsewhere adverted to the analogy which exists between the operation of the nervous power and that of electricity. The influence of volition is transmitted instantaneously from the brain to the muscles, and impressions on the sentient extremities of the nerves are communicated to the brain, the nerves themselves answering the same purpose as the conductors of an electric apparatus. The agent in both instances is invisible, intangible, and

* Lectures illustrative of certain Local Nervous Affections, page 11.

known only by the effect, which it produces; and, these things being considered, it seems to be not contrary to what analogy would lead us to expect, that in like manner as the electric force, generated by chemical decomposition at one end of a metallic wire, directs the needle of a telegraph, or causes the explosion of gunpowder, at the other extremity, so a substance, such as the hydrocyanic acid, which powerfully affects the vital properties of the part to which it is applied, should be capable, through the medium of the nerves, of disturbing, and even of arresting, the functions of the brain.

I would not, however, be understood as affirming that the facts which I have now stated, or the suggestions which I have offered, are to be regarded as conclusive. Further observations may be required before the question is finally determined. In the meanwhile, in this as in numerous other instances, (which occur not much less frequently in the strict pursuits of science than in the common affairs of life,) some individuals will be inclined to one opinion, and others to another, accordingly as one or another order of facts may more especially have engaged their attention.

As connected with this subject, I may take the opportunity of noticing a circumstance which came under my observation as long ago as the year 1809, and which seems to show that the influence of certain poisons admits of being propagated to distant parts by the mere continuity

of structures, independently either of the nerves or of the vascular system. A man was admitted into St. George's Hospital who had been bitten by a rattle-snake on the thumb and finger of one hand. The immediate effect of the injury was great constitutional disturbance, from which he gradually recovered. Then the limb became inflamed, and there was sphacelus of the cellular tissue as high as the middle of the arm, followed by suppuration. But, in addition to all this, there were large spots of extravasation of blood in the subcutaneous texture, far beyond the limits to which the inflammation had extended. These were observable on the back as low as the hip-joint, and on the anterior part of the chest, over the *pectoralis major* and *serratus anticus* muscles, but they were confined altogether to the side of the body on which the injury had been inflicted, not extending over the median line. The case has been recorded by Sir Everard Home, in the Philosophical Transactions for the year 1810.

Note C. (Page 46.)

The following observations are extracted from the 27th volume of the Medical and Physical Journal, in which they were inserted, as I have

reason to believe, in consequence of a communication from the late Mr. John Pearson, who, as it appears, had made some experiments, showing the rapidly fatal effects of the poison extracted from bitter almonds, in the year 1806.

"In following Mr. Brodie through his investigations we have been struck with the effect of the essential oil of almonds. The improbability (*à priori*) of finding such a deleterious substance in that fruit, its instantaneous and vivid effects on the vital principle, not leaving time for absorption, but travelling like the electric fluid along the nerves to the brain, are circumstances not less important than they are astonishing. A gentleman who had the hardihood to apply a particle of the oil to his tongue describes its effect to be like that of a blow on the brain. To the shock succeeded instantaneous mental confusion, and the rapid approach of insensibility. The accuracy of Mr. Brodie both in observation and in detail is remarkably borne out by the coincidence of his experiments with those of Mr. John Pearson, of Golden Square, in 1806."

In addition to what I have stated elsewhere, it is worthy of notice that the effluvium of the essential oil of bitter almonds partakes of the deleterious quality of the oil itself, as was proved by giddiness and other uncomfortable sensations experienced by myself and some of my pupils, who assisted me in my researches, after we had been for some time exposed to its influence.

I may take this opportunity of observing that

the strength of the oil, as a poisonous agent, differs considerably. I suspect that the first portion of the oil, which during distillation passes into the receiver, contains more of the poison than that which is obtained afterwards; and that this explains why the oil which had been prepared as an article of commerce was much less active than that which had been specially prepared for the purpose of my experiments.

Note D. (Page 49.)

The mode of death in which the cessation of the action of the heart precedes the cessation of that of the muscles of respiration, is not so frequent as that in which the circulation continues after respiration has ceased. I found, however, that some poisoned arrows brought from Mozambique by the late Mr. Salt, produced death in the first of these two ways, thus resembling in their effects the Upas Antiar and the infusion of tobacco; and Dr. Christison has shown that the oxalic acid in a diluted state has, when taken into the stomach, a similar mode of operation.

It is worthy of notice that when death is suddenly produced by exposure to intense cold, the phenomena which occur are very similar to those which follow the exhibition of the above-

mentioned poisons. The action of the diaphragm continues after that of the heart has ceased, and the animal dies with scarlet blood, in the left cavities of the heart and in the aorta. This fact has been ascertained by Dr. Chossat and myself, and may be of some practical importance, inasmuch as it has been proved by one of my experiments with the infusion of tobacco, that under these circumstances there is a possibility of the action of the heart being restored, which never can happen when the circulation of dark-coloured blood has continued after respiration has ceased.

In the true *angina pectoris*, depending on ossification of the coronary arteries, it is probable that death takes place in the same way. The supply of scarlet blood to the muscular structure of the heart by these vessels is sufficient for ordinary purposes, but when, under the influence of mental emotion, or from other causes, the heart is required to make any unusual exertion, the supply is insufficient, and its action ceases. Mr. Hunter, however, who laboured under this disease, supposed that on one occasion he kept himself alive by a forced or voluntary act of respiration, after ordinary respiration had been suspended.

The most marked symptom of ordinary syncope is the failure of the pulse, and it is not improbable that in some instances the heart entirely ceases to act, though it resumes its action afterwards.

Note E. (Page 59.)

Since these experiments were published, I have discovered another effect of the woorara, which illustrates in a striking manner the powerful influence which it exercises over the nervous system, affecting the nerves of motion not less than it does those of sensation. It is well known, that if the spinal chord be divided transversely, although the parts below the division are deprived of sensibility, nevertheless powerful contractions of the muscles are produced by the mechanical destruction of that portion of the spinal chord from which their nerves are derived. But if an animal be rendered insensible by the woorara, and the circulation be then maintained by artificial respiration, the same effect is not produced by the same injury, the muscles being as wholly unaffected as they would have been in the dead animal.

It has been shown by the Abbé Fontana, that the poison of the viper, virulent as its effects are when applied to a wounded surface, may be taken into the stomach with impunity. A similar observation respecting the woorara has lately been made by M. Bernard and M. Pelouze. The same thing occurs in the case of some animals as to opium. I injected a strong watery solution of opium into the stomach of a rabbit, but no poisonous effect was produced; although a similar

solution injected into the cellular membrane occasions stupefaction and death. I presume that the opium is digested by the stomach of the rabbit, although it is not so by that of a carnivorous animal.

Some poisoned arrows from the Andaman Islands, with which I was furnished by the late Mr. Wilkins, were found to act precisely in the same manner as the woorara; and I conclude that all the more concentrated and more powerful narcotic poisons have a like operation, and destroy life by paralysing the muscles of respiration, without immediately affecting the action of the heart. But it is otherwise when, either from the more feeble action of the poison, or from it being more diluted, or more gradually administered, the period of death is protracted. In the case of a patient, who survived the being poisoned by the tincture of opium for more than twenty-four hours, the whole of the vital powers seemed gradually to become exhausted; and from the failure of the pulse, it appeared that the contractions of the heart must have ceased nearly at the same time with those of the muscles of respiration.

The phenomena of death from drowning, strangulation, or confinement in an atmosphere in which there is too large a proportion of carbonic acid, are very similar to those produced by a narcotic poison. The heart continues to act after the animal is apparently dead, and he may be restored to life, if artificial respiration be had

recourse to before the circulation has ceased. It is worthy of notice, that under these circumstances the animal does not recover his sensibility, nor his power of locomotion, until some time after he has begun to breathe spontaneously; and hence we may conclude, that the transmission of dark-coloured blood to the brain has not merely a negative but a positive effect, in fact operating on it in the same way as a narcotic poison. In some instances the affection of the brain thus produced continues so long that ultimate recovery does not take place; the animal (if I may be allowed the expression) dying a second time, in consequence of the muscles of respiration again ceasing to perform their office. These observations are applicable to the human subject, as well as to the inferior animals.

I have elsewhere * recorded some experiments, showing in what manner death is produced by the discharge of an electric battery. It is not out of place for me to refer to them on this occasion, as the phenomena bore a very near resemblance to those produced by the more powerful narcotic poisons.

* Lectures illustrative of various Subjects in Pathology and Surgery, 1846, page 101.

Note F. (Page 68.)

When these observations were communicated to the Royal Society, there was not the same uninterrupted intercourse with other countries as at the present time; and hence it was that I was not acquainted with the very important researches respecting absorption by the veins, an account of which had been previously given to the Institute by M. Magendie. Holding the opinion, which was then universally received, that absorption, in the physiological sense of the word, is performed only by the lymphatic vessels, I was led to believe that as the poison did not enter the circulation by the latter channel, it could have done so only by the wounded veins. With the knowledge of the facts established by M. Magendie, it is scarcely necessary for me to add, that I do not at present doubt that the division of the veins is not necessary to the admission of the poison into these vessels.

Although it is satisfactorily proved that certain poisons affect the system through the medium of the blood, there are nevertheless some facts which seem at first to be incompatible with this view of the subject. I refer more especially to the experiments of M. Magendie, in which he found that when the blood of a poisoned animal had been transfused into the bloodvessels of another animal, the latter was in no degree affected by it.

The probable explanation is, that the poisoned blood of the one animal was rendered innocuous by being diluted with the healthy blood of the other.*

In the second volume of the Philosophical Transactions there is the history of an experiment, in which several ounces of the blood of a mangy dog were transfused into the jugular vein of a healthy dog, without the latter being affected by the disease. But, on the other hand, I was informed by the late Professor Coleman, of an analogous experiment which he had made, but in a more complete manner, with a different result. Professor Coleman's statement was, that having abstracted so much blood from a sound horse as to induce either complete syncope, or a state approaching to it, he transfused into him the blood of a glandered horse; the result being that the sound horse recovered from the syncope, but some time afterwards was affected by the glanders.

* The same observations may be applied to some experiments recorded by Dr. Addison and Mr. Morgan, in their work on poisons. Indeed it is evident from the way in which these experiments were conducted, that the transfusion must have been very imperfect, and that if any blood actually passed from one animal to the other it could have been only a very small quantity.

Note G. (Page 80.)

The cat which was resuscitated, after being apparently dead under the influence of the woorara, was given to a friend, and lived, as I have been informed, for some years.

Not long after this experiment was made, I repeated it on an ass, with the assistance of Professor Sewell, at the Veterinary College. The animal lay in a state of total insensibility (the lungs being inflated by means of a pair of bellows and a tube introduced into the trachea) for more than an hour. He then recovered, and seemed to suffer no inconvenience afterwards. The poison used on this occasion had been given to me by Mr. Waterton, who had himself brought it from America. The ass had been purchased for the purpose of the experiment, by the late Duke of Northumberland (then Earl Percy), and was afterwards given by his Lordship to Mr. Waterton, in whose island he was allowed to range, and where he was alive many years afterwards.

The method of resuscitation, which is here described, is evidently applicable to all cases of apparent death, in which the action of the heart, so as to maintain the circulation, continues after respiration has ceased. The success of the treatment depends, 1st, in cases of poisoning, on the

dose of the poison, a limit to the period there being during which life can be maintained by means of artificial respiration; 2ndly, on the inflation of the lungs being carefully made*; 3dly, on the animal being kept in a temperature of not less than 85 or 90 degrees of heat of Fahrenheit's thermometer. This last precaution is of course a matter of greater importance where the animal is of a small size (as in the case of a cat or rabbit), than where it is larger; still it is not to be neglected even in the case of the human subject; otherwise the animal heat gradually diminishes until it reaches that point at which the action of the heart can no longer be maintained, when we have the singular result of an animal perishing from cold in the ordinary temperature of the atmosphere. I have not myself known the circulation to continue where the temperature of the interior of the thorax has been below 78° of Fahrenheit; but an experiment is related by Dr. Chossat, in which it had fallen still lower.

It is needless to multiply examples of the kind. I am, however, induced to record the following experiment, as it derives a peculiar interest from the circumstance of the use of ether and other anæsthetic agents having been lately introduced into the practice of surgery.

* On this subject I have offered some observations elsewhere. See Lectures illustrative of various Subjects in Pathology and Surgery, page 7.

February 5. 1821.

A guinea-pig was placed under the bell-glass employed in my experiments on animal heat. The bell-glass was left open above, while a small retort containing sulphuric ether was adapted to the tube communicating with the lower part of the apparatus. A lamp was placed below the retort, so as to make the ether boil. The vapour of the ether thus became mixed with the air in the bell-glass, a portion of it becoming condensed on the inner surface of the latter, and on that of the wooden stand on which it was placed.

In two minutes after the experiment was begun the animal moved about briskly, as if affected by the first symptoms of intoxication. In two minutes more he lay on one side in a state of insensibility, but still breathing. He continued in this state, breathing at longer and longer intervals, for six minutes, when respiration had entirely ceased. After two minutes more he was removed from underneath the bell-glass. Though he was apparently dead the heart could be felt beating feebly through the ribs. An opening having been made in the trachea, the lungs were now artificially inflated. Only a few seconds had elapsed before there was a spontaneous effort to breathe, and the pulsations of the heart were more distinct. When the artificial respiration had been kept up for some minutes longer it was discontinued. The animal now breathed naturally, and gave some slight indica-

tions of sensibility when touched. This was followed by a tremulous motion of the limbs, and soon afterwards by complete recovery.

Note H. (Page 91.)

The researches of modern toxicologists, and the application of a more exact chemistry to the detecting of poisons, not only in the alimentary canal, but in the different tissues, and even in the blood itself, afford a sufficient contradiction of the observation, that where death has been produced by arsenic "but little is to be learned from the examination of the contents of the stomach." The fact is, that, in pursuing these investigations, the object which I had immediately in view was to determine the operation of poisons on the various vital organs, and the mode in which they occasion death; and that my attention was only incidentally directed to a subject, which, notwithstanding its great practical importance, is not that which is of the greatest interest to the physiologist.

The world is deeply indebted to those whose successive labours have brought the art of tracing the existence of these mysterious agents after they have done their work of death, to the state of comparative perfection which it has now attained, and especially to M. Orfila, with whom

these inquiries may be said to have originated, and who still, after the lapse of forty years, continues to prosecute them with not less zeal than formerly, and with no less success than that which rewarded the efforts of his early life.

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